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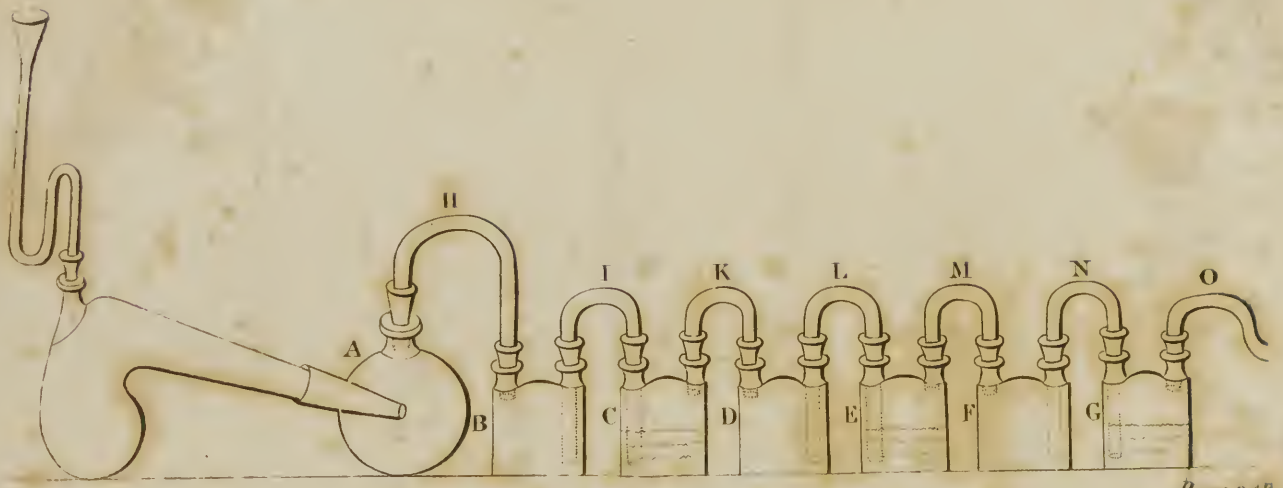
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MEMOIRS

OF THE

COLUMBIAN CHEMICAL SOCIETY,

OF PHILADELPHIA.

VOLUME I.

PUBLISHED BY ISAAC PEIRCE,

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1813.

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District of Pennsylvania, to wit:

SEAL.

BE IT REMEMBERED, That on the Sixth day of October, in the Thirty-seventh year of the Independence of the United States of America, A. D. 1813. ISAAC PEIRCE of the said District, hath deposited in this office the Title of a book, the right whereof he claims as Proprietor in the words following, to wit:

Memoirs of the Columbian Chemical Society, of Philadelphia, Volume I.

In Conformity to the Act of the Congress of the United States, intituled, "An Act for the Encouragement of Learning, by securing the Copies of Maps, Charts and Books, to the Authors and Proprietors of such Copies during the times therein mentioned." And also to the Act, entitled, "An Act supplementary to An Act, entitled "An act for the Encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies during the Times therein mentioned," and extending the Benefits thereof to the arts of designing, engraving, and etching historical and other Prints."

D. CALDWELL,
Clerk of the District of Pennsylvania.

PREFACE.

IN presenting to the public the first fruits of the Columbian Chemical Society, it may not be amiss, to give a short history of its rise and progress.

The Columbian Chemical Society was founded in the month of August 1811, by a number of persons desirous of cultivating chemical science, and promoting the state of philosophical enquiry. And although the number of members has not increased rapidly; yet, from the zeal manifested by the few who compose the Society, in investigating scientific topics, there is every reason to believe, that the period is not far distant, when this institution will rank high in respectability.

From the nature of the subject to be investigated, it followed, that essays would frequently be offered by its members. Hence arose that multiplicity of dissertertations from which this selection

has been made. The appearance, however, of some papers, whose relation to chemistry, is scarcely, if at all perceptible, prompts us to make one remark. One of the articles of the constitution expressly declares, that communications may be on subjects either chemical or philosophical.

The contents of the volume, are chiefly original, partly speculative, and partly practical. The great variety of topics investigated, the novelty of many of the essays, will, it is hoped, render the whole, somewhat interesting. In addition to the papers of the Society, it has been deemed proper to publish the Constitution, together with a list of officers, members, &c. &c.

CONSTITUTION

OF THE

COLUMBIAN CHEMICAL SOCIETY.

Deeply impressed with the importance of an association, devoted to the investigation of scientific subjects, we the subscribers mutually agree to form ourselves into a society, to be governed by the following Regulations:

1st. That this society shall be known by the title of the Columbian Chemical Society of Philadelphia.

2. The officers shall consist of a President, two Vice Presidents, a Corresponding Committee, secretary, Treasurer, and Orator, to be elected by ballot, in the month of January in each year, and in case of vacancy by death or resignation, it shall be supplied by special election.

3. The duty of the President shall be to preserve order, regulate debates, and conduct the business generally.

4. The Vice Presidents shall preside in absence of the President, and in case of the absence of both Vice presidents, a chairman shall be elected *pro tempore.*

5. The Secretary shall keep fair minutes of the transactions of the society, preserve all papers, give due notice of all meetings, call the roll at the opening and close of each meeting and mark down absentees.

6. The Treasurer, giving security, shall receive in trust for the society, the funds thereof, dispose of the same on order, keep regular entries, and once in 3 months, produce his accounts for the inspection of the society. In case of resignation, he shall transfer to his successor, on order from the President, such of the society's property, as he may possess.

7. An oration on some chemical subject shall be delivered within two months after the commencement of the medical lectures in the University of Pennsylvania, in each year.

8. The stated meetings of the society shall be held on the third wednesday of every month.

9. Every member shall be fined $12\frac{1}{2}$ cents for absence each roll, unless satisfactory reasons be offered.

10. Any member behaving in a disorderly manner, shall be expelled by consent of $\frac{2}{3}$ of the members present.

11. Any person desirous of membership shall be proposed and seconded at one meeting, and be ballotted for at the next; previous to the election, he shall read an original essay on some chemical subject on which any member may speak not more than ten minutes. Every candidate shall have two thirds of the votes of the members present in his favor to procure election, and shall pay 2 dollars into the hands of the treasurer.

12. The society shall appoint, once in each month, some member to read an original chemical essay, for neglect of which, the member so appointed, shall be fined one dollar.

13. No member shall be compelled to read more than one dissertation in the course of six months.

14. Any member being elected to office, and refusing to serve, shall be fined one dollar.

15. The Constitution shall be revised annually, and the revision shall take place in January; no alteration shall be made in it without the consent of two thirds of the members present.

16. Six members shall constitute a quorum and may proceed to business.

17. All bye-laws not inconsistent with these articles, shall be equally binding.

Officers of the Columbian Chemical Society.

Hon. Thomas Jefferson, Esq. *Patron.*

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TO THE READER.

Owing to the difficulty that we necessarily laboured under, in commanding the attention of so many different authors to the correction of their essays in the proof sheets, as well as to the obscure hand in which some of them were written, many typographical errors have escaped; but we hope that generally they are of such a nature, as not to render the meaning ambiguous; it is upon this ground we claim a portion of the authors' as well as the reader's indulgence.

THE PUBLISHER.

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MEMOIRS

OF THE

COLUMBIAN CHEMICAL SOCIETY.

Remarks on the Phlogistic and Antiphlogistic Systems of Chemistry. By THOMAS D. MITCHELL. M. D. *Fellow of the Academy of Natural Sciences of Philadelphia. Honorary member of the Columbian Chemical Society, &c.*

THE grand dispute, between the Phlogistians and Antiphlogistians, appears to consist in this: the former believed, that in combustion, the body burnt, parted with something, supposed to be Phlogiston or the principle of inflammability. They imagined, that in order to restore its combustibility, it must again receive the principle of inflammability, in other words, that Phlogiston must re-enter its composition. The Antiphlogistians with more philosophical reason, supposed, that in combustion, the burning body, instead of losing, combined with something, and, that in order to restore the combustibility, this substance, which had been combined during combustion, must be separated. This peculiar matter was oxygen.

Numerous have been the advocates of these very opposite points. They have laboured hard in the field of experiment and wandered far in the mazes of speculation. But, in all disputed subjects, the balance finally preponderates in favour of one or the other sentiment, and in the case before us, the Antiphlogistians have proved victorious. Their leader, the immortal Lavoisier has been crowned with laurels, durable as fame. Before him fell the stern votaries of the Phlogistic system. His theory, says Dr. Black, is bold and ingenious, and science has cause, greatly to deplore the death of so eminent a philosopher. Had this great man escaped the blast of tyranny and eluded the storm of despotism, the science of Chemistry would have shone with tenfold splendor at this day. Extravagancy in speculation would have been silenced by the cool deliberation of reason.

Notwithstanding the apparent death blow that the Phlogistic system received, there appears some probability of a renewal of the old dispute. The various researches of Davy have given rise to doubts respecting the tenable nature of the whole Antiphlogistic system. In consequence of these investigations, some chemists of the present day are desirous of establishing as an axiom, the existence of a principle of inflammability in all inflammable bodies. This, in fact, is very little, if any thing different from the Stahlian theory.

In my opinion, however, there is no more just ground for the admission of the Phlogistic system at this day, than there was in the time of Lavoisier.

Place the practical inquiries of this philosopher in one scale, together with reasonable speculation, and in the opposite scale place all the vague hypotheses that have ever been or are now tolerated, and let candor decide.

We are told that Hydrogen has been proved to exist in Carbon, Sulphur, Phosphorus, &c. as an essential component part. Some experiments have been made to prove that Carbonated Hydrogen could be obtained by exposing to great heat, a quantity of charcoal perfectly freed from water. From such experiments, the inference has been made, that hydrogen necessarily existed in charcoal. The same was done with regard to sulphur, phosphorus, &c.

In these experiments, we have no doubt that accuracy was observed so far as concerned the process. But we have good reason to believe, that the nature of the products was misunderstood.

It may be thought strange, that in a subject of so much importance, I should resort to but one experimental proof in favour of the position I attempt to support. But when the nature of that single proof and its obvious importance is discovered, it will appear to swell into a host of arguments.

In order to bring the reader fairly into an acquaintance with my design, I must state some experiments made by the late Dr. Woodhouse. These were designed as insuperable barriers to the Antiphlogistic system. For the professor candidly acknowledges his conviction of the fallacy of that system, as will appear by a perusal of his

"Appendix to Parkinson's Chemical Pocket Book."* The Doctor performed an experiment, much celebrated on account of the zeal manifested in it, to obviate certain objections that might have been urged against his inferences. He mixed charcoal with scales of iron, both freed from water by previous exposure to a red heat. His object was, by placing the mixture in a proper vessel with convenient appendages, to determine what gasses would result. According to the Antiphlogistic theory, no hydrogen could be obtained in this process, yet the Doctor tells us that a gas containing hydrogen was procured. He goes further and asserts, that he has procured inflammable air, (meaning hydrogen) by heating a mixture of flowers of zinc and charcoal. From these results the Doctor formed his objections to the new system of chemistry; and in fact, had his inferences been correct, this system must have sustained some injury. Admitting the correctness of the Doctor's experiment, some chemists would gladly seize the opportunity of attempting to prove that the supposed hydrogen came from the charcoal, others would look to the iron for this product, and humbly hope to restore to Phlogiston the place it once occupied. Among the latter was Dr. Priestley. This philosopher, though a true favorite of genius, fell into the grossest errors. He performed experiments very similar to those already mentioned as performed by Dr. Woodhouse. The products

* I am aware, however, that the Doctor afterwards recanted, and resumed his attachment to the new system.

were similar and Dr. Priestley ventured to account for the supposed appearance of hydrogen, by the evolution of Phlogiston. To this effect, he wrote several pages, some of which may be found in the 1st. and 2nd. vol. of the New York Medical Repository. The results of the experiments, although fallacious as will hereafter appear, called forth the energies of many respectable chemists. They admitted, very improperly too, the the correctness of Dr. Priestley's experiments and disputed his inferences only. Mr. Adet, a French chemist, in an answer to Dr. Priestley, endeavoured to account for the appearance of Hydrogen in the experiment we have mentioned, by saying that charcoal very often contained this substance. Thus we see that experiments, which we hope to show were fallacious, and inferences which of course must be unfounded, have involved in them the correctness of the famous Antiphlogistic system.

But the opposition thus excited, ephemeral in its nature, soon vanished. Anxious to efface the blot necessarily cast on the new theory by such investigations, Mr. Cruikshank of Woolwich ventured to repeat the experiment which had produced such effects on the mind of Dr. Woodhouse. It will be also recollected, that this experiment is similar to some of Dr. Priestley's made with a view to the support of the Phlogistic system.

Mr. Cruikshank accordingly performed the experiment and the result seemed at first to confirm the inferences of Dr. Woodhouse. An inflammable

gas was produced as had been asserted. But the decision of a scientific point, highly important in its nature was not to be rested on the evidence of mere appearance. Inquiry was therefore urged still further, and an analysis of the gasseous product was effected. And here let it be remarked, that while truth was about to erect her standard, a valuable accession was also made to the fund of Chemical science. The product was found to be a substance unknown at that day, and has ever since been reputed as the discovery of Cruikshank. The inflammable air of Woodhouse proved to be a compound of oxygen and carbon, 59 of the former to 41 of the latter in the 100. It is called Gasseous Oxide of Carbon, and though inflammable, it contains no hydrogen.*

Here then we perceive the effect of delusion in experimental inquiries where the mind is biassed in favour of any opinion. The result of this experiment is perfectly conformable to the mode of reasoning pursued by the Antiphlogistians.

Now, let us reflect a moment on the consequences of a single mistake in experimental researches. The error of Priestley and Woodhouse, did not operate merely on themselves, but like an *ignis fatuus*, it led thousands astray. Yet we see that the indefatigable labours of one man have sufficed

* It may not be amiss to mention, that Dr. Priestley has attempted to reconcile Mr. Cruikshank's discovery with his favourite system; but the attempt, in my opinion, is altogether ineffectual. Dr. Priestley's paper on this subject may be found in the 5th. vol. of Med. Rep.

to expunge this mistake from the minds of thousands. His correctness is admitted by all who have repeated the experiment.

When we are told that carbonated hydrogen was procured by heating charcoal freed from water, we have just cause for believing that no such product was obtained. The same may be asserted with regard to phosphorus, sulphur, &c. The probability is, that in all these cases, oxides were produced, in a gasseous form, as in the case of gasseous oxide of carbon. If Priestley, Woodhouse and others, among the most celebrated chemists who have ever lived, were deceived merely by mistake and from that deception were led to adopt false theories, we need not wonder that others less famous in the scientific world should be deluded in a similar manner.

Much has been said relative to a principle of inflammability in inflammable bodies; but admitting its existence as a possible thing, I see no necessity for it. No body can undergo combustion without the presence of oxygen, even hydrogen itself will not burn without it, and yet this is urged as a principle of inflammability.

In the experiment as performed by Woodhouse, Cruikshank &c. the iron was revived, consequently rendered combustible. If the Phlogistic system be correct, the iron must have received an accession of the inflammable principle or hydrogen, but so far from this, not a particle of hydrogen was present. The iron was revived and rendered combustible, not by the restoration of the principle

of inflammability, but as the beautiful system of Lavoisier justly teaches us, by an abstraction of its oxygen. By this fact, also, one of the conclusions made by Mrs. Fulham in her essay on combustion, is completely overturned. That writer has asserted, that no metallic reduction can take place, except hydrogen be present. But we have shown that iron has been reduced, independent of the agency of hydrogen.

It will be evident to all, that I have not aimed to prove the non-existence of an inflammable principle ; all I have attempted is to show the fallacy of means hitherto employed to prove that there is such a principle. It remains therefore with its advocates to bring forward better evidence in support of their position. I do not believe it of importance to make any effort to disprove this sentiment, because, there appears to me, no necessity for a principle of inflammability. And I believe that all important phenomena can be accounted for independent of such an agent.

I shall now conclude with a few general remarks. And in the first place, I would observe, that while the doctrine of an inflammable principle appears to me incorrect, it seems likewise unphilosophical. And while I say this, I am as willing to admit the error so often made, in the use of the term, principle of acidity. Both are alike improper. It would be just as philosophical, when speaking of a neutral salt, to assert, that the acid constituted the principle of neutrality, as, that it resided, exclusively, in the alkali. How absurd

does this appear! shall we then be excused, when we say that such a body is combustible, if we assert that the principle of inflammability belongs to one agent? or, speaking of an acid, shall we be allowed, to call one of its constituents, the acidifying principle? I grant that acids are found generally to contain oxygen, but does this prove it to be the efficient cause of acidity? Pray, let me ask, where would be the lists of acids, if the bases as they are styled, were not called into action? They are as much principles of acidity, as oxygen, and it is an abuse of terms, a misapplication of words, to say that this or that is the principle of inflammability or of acidity. Inflammation and acidity are effects resulting from the action of relative causes and not attributable to a single agent or principle.

We do not seek for a secreted something in the base of an acid, to make it assume the characteristic of acidity, or to render it different from any other acid. The great difference in obvious qualities is sufficient to account for the immense variation observed. So, in combustion; oxygen and a combustible body are present, and I ask where is the ground for searching an inflammable principle, a hidden, ideal? the difference in appearance which is observed on burning phosphorus and a piece of wood is readily explained, by the obvious dissimilarities exhibited by these substances. Why not admit, that phosphorus and iron, considered either as simple or compound, will burn in contact with oxygen, independent of the assistance of an agent, called forth by fancy, when we grant that barytes,

lime or any other base may form a neutral salt when an acid is added, without going in quest of an unknown non-entity.

I may be charged with, vainly speculating to no purpose, by some who are prejudiced in favour of one or the other system. But as we are irresistably compelled to think and as I have been accustomed to reflect on this subject, it would have been an act of injustice to my feelings to have refrained from these observations. They are therefore, respectfully submitted to the candid examination of all and I trust they may be at least so far useful, as to excite a more general inquiry on subjects of this nature.

Wednesday, October, 23*d.* 1811.

AN ENQUIRY

Into what circumstances will warrant us justly to reckon any substance a principle of a common property of any set of bodies. *By* FRANKLIN BACHE. *Member of the Columbian Chemical Society.*

To render a substance a principle of any common property which may exist in any set of bodies, we conceive it not only requisite to prove that it exists in all the bodies possessing the common property; but that when united in a sufficient proportion with any body simple or compound, which is susceptible of the common property, that it should appear; thus, to apply this definition to the particular investigation, which is to be entered into in the present enquiry, (which is, upon what ground we shall be warranted in believing any substance, a principle of acidity, in Chemistry,) it will be not only necessary, that we should prove that any particular substance exists in all bodies possessing the property of acidity, but also, that when presented in sufficient quantities, under proper circumstances, to any substance, capable of acidification, acid properties should appear.

As Hydrogen is the only substance which has contended the palm with Oxygen, for being the principle of acidity, our enquiries shall be con-

fined to weighing the respective claims of these two important substances to that honour: although it is an hypothesis that has raised Hydrogen to the exalted station of a competitor of Oxygen, yet, for the purpose of refuting its pretentions more completely, we will suppose that hypothesis a well ascertained fact. The hypothesis to which we allude, is that which supposes that hydrogen is a constituent in all acids; from which the conclusion is drawn by some, that, as the belief that oxygen was this principle, was founded upon the circumstance of its presence in all acids, therefore hydrogen has an equal claim with oxygen. But we contend that the very circumstance, of the *possibility* of two substances existing common to any set of bodies (and we not only think it possible, but likewise very probable,) should have taught the advocates of this doctrine, that that was not a proper definition, which was founded on the circumstance merely, of a substance being common to any set of bodies; and why? the reason is obvious; for if it were a proper definition, then two substances could independently of each other be capable of producing acidity, which is impossible.

Such were the reasonings which led us to seek for some other more complete definition; which we feel confident we have discovered in that which we mentioned in the beginning of the enquiry.

Let us enquire how the two substances Oxygen and Hydrogen agree with the definition: with

the first part, we have admitted they both agree, and if the decision rested upon this point alone, the matter would be completely ambiguous; but this is far from being the case, as something further is necessary before we can decide the question, and this is, whether acidity is produced when either substance is presented in sufficient quantities under proper circumstances to any other substance, either simple or compound which is susceptible of acidification.

Let us now enquire if Hydrogen produces acidity when united to any other substance capable of acidification; we contend that in no case it does even in appearance, except in sulphureted Hydrogen; in the consideration of which substance, the force of analogy, the possibility of its having Oxygen in its combination from the nature of the ingredients necessary for its production, as being muriatic acid—water—iron—and sulphur, and from the small number of analyses which have been yet made, all conspire to make us believe that oxygen must be present in this substance; in short when we consider that Hydrogen can unite with two analogous substances, in the form of Carbureted and Phosphureted Hydrogen gases without producing acidity, we are led involuntarily into the belief, that that substance which is called sulphureted hydrogen, might with more propriety be considered as a compound of muriatic acid gas and sulphuric acid, or if the conjecture be prefered, as Muriatic acid gas, holding a small quantity of sulphur in solution.

But yet lest there should be still some who conceive that the admission of the presence of hydrogen in all ternary acids, and in one binary in which Oxygen is solely the other constituent invalidates the belief in Oxygen as the principle of acidity, we shall enter into a set of arguments fitted for persons of this mode of thinking; for this purpose it will be necessary to premise two things. 1st. That we shall confine ourselves to the consideration of such anumber of binary acid compounds, as a belief in the presence of hydrogen in all metals, as well as in sulphur Carbon and Phosphorus will necessarily limit us to—the reason of this limitation will be obvious, when it is considered, that these compounds are the only acids in which both Hydrogen and Oxygen do not exist, or if they do, not in combination with any other substance 2nd. We must reason upon the supposition that a certain matter is decided in a certain way, which we candidly admit is not decided in any way, or in other words we must suppose that hydrogen is the base of the Muriatic acid. At first view it may not seem that the decision of this point has any very direct relation to the subject we are discussing; but we hope to shew in the sequel, that the assumption of this circumstance as a fact, is not necessary only, but indispensible in the decision, whether oxygen can or cannot be demonstrably proved to be the principle of acidity.

Let us now consider the binary acid compounds limited, as we have before stated.—They are

1st. Oxygen and Nitrogen forming Nitric Acid.

2nd. Oxygen and Hydrogen forming Muriatic Acid.

Here it is perceived are omitted all the vegetable acids, and the five Metallic, together with the Sulphuric Carbonic and Phosphoric acids; the former, because they are known to contain hydrogen, the latter, because they contain it by the supposition.

Let us consider the first acid mentioned, namely the Nitric: here then by this chemical combination, a vast change has taken place in the properties of the bodies, concerned in the combination; the most striking of which is, their sourness, or what is usually termed their acidity, now this acidity must either arise from the Oxygen, or the Nitrogen or from both; but it does not arise from both, for if it did, Oxygen alone could not produce the same effect, with another body totally different in its properties as Hydrogen, with which it forms Muriatic acid; neither does arise from the Nitrogen, because if it did, this substance when united to Hydrogen would form an acid, but on the contrary, it forms a substance having distinctly metallic properties, and known to be the basis of volatile alkali ammoniac. So that as we have disproved two of the three ways in which the acid properties could arise, we may safely conclude negatively, that the third, which supposes oxygen, is the cause of the acidity. But can we come to the same conclusion by positive reason-

ings? we answer, most certainly; thus if it be the oxygen which causes the acidity, it will, when presented to Hydrogen form an acid, but is this the case? yes, for in this combination in a certain proportion it forms the Muriatic acid. So that we arrive at the same conclusion, whether we reason positvely or negatively.

These reasonings will also have a tendency to demonstrate the fallacy of an opinion, most ingeniously supported by our President at a late Meeting of the Society; he says, (speaking of the dispute concerning the principle of acidity) he conceives it to be completely unphilosophical, and that in no case can an effect produced by a chemical composition be reasonably refered to one of the ingredients only, but that they must both necessarily concur, in producing the properties generated; now we will venture to say that these erroneous deductions of the gentlemen to have arisen, entirely from a hasty analogy, referring what happens in combinations, forming neutral salts, to the cases of combinations, forming acids. In the former case neutralization is produced, which we readily admit inculcates the idea of the concurrence of both substances in producing the effect; but Mr. Mitchell reasons from analogy, that acidity is produced in the same manner; and here is perceived an assumption that can be by no means admitted, which is, that if both bodies, in one chemical *combination* concur in producing a property generated, that this must be the case in the *generation* of any *other property* in any other

combination. But so far from believing this to be the case, it is our opinion when reasoning abstractedly, that the properties generated by any composition, may be referred with equal propriety to *one* as to *both* or all ingredients employed ; there is nothing in the nature of the case, which would lead us to think otherwise ; and we may add that this belief is not only reasonable and in the nature of things, but that it is completely consonant with fact: we have said before that the properties generated in any chemical union must necessarily be referred either to both or all the constituents taken collectively, or to one of them alone; how can we reasonably say that one of the possible ways must always take place, to the exclusion of the other ; besides, the circumstances, from their nature, attendant on any chemical union, will inform us when the changes are produced by the joint action of both, or whether the new properties are to be entirely referred to some one of the ingredients : for instance, in the formation of a salt a negative property is generated ; a property seeming to indicate both substances to have advanced half way in producing the effect ; but in the formation of an acid, the matter is completely otherwise ; for there we have qualities generated, obviously and decidedly of a positive nature ; neither is there any thing which inculcates the idea, that the substances have both concurred in producing the effect, nor that any thing intermediate in properties has been formed.

Thus we have rendered it probable that both cases can exist; that in acids but one, that in salts both ingredients concur in producing the respective effects. But as we have proved by a certain method to demonstration that oxygen alone is the cause of acidity, a good way to test the rule of reasoning, upon which those conclusions are founded, would be to apply it to the case of a neutral salt, to see to what conclusion it will there lead us, as it respects the cause of the neutrality —for this purpose we shall select for consideration the muriate of soda; now it is required to find from what source this body derives that property called neutrality; it must necessarily arise either from the muriatic acid or the soda, or from both; but it does not arise from the muriatic acid alone, for if it did, a salt should be produced in all its combinations with other bodies, whether they be similar or dissimilar, when they unite in an indefinite proportion, which is not the case, as the consideration of the nitro-muriatic acid will abundantly prove; neither does it arise from the soda for exactly the same reason; therefore if the property generated arises neither from the acid nor the alkali individually, it must necessarily arise from both—this is a negative deduction, but we arrive at the same conclusion, when reasoning positively; thus, if the property generated arise from both, then either body will not be able to produce the effect when united to any other dissimilar body, which is exactly the case; therefore it does arise from both—or we might

prove the same thing by supposing it did not arise from both, which would necessarily lead us into an absurdity. Thus it is perceived, that whether we reason positively or negatively, or by a combination of both methods, we arrive at the same conclusion.

We shall conclude by enumerating some general propositions, demonstrated in, or deduced from, the foregoing views of the subject.

1st. That the belief that hydrogen exists in all ternary acids, as well as oxygen, does not invalidate the opinion that oxygen is the principle of acidity; or in other words, we need not reject those arguments which make it probable that there exist but three elementary substances, from a fear that they have a tendency to subvert the belief that oxygen is that principle.

2nd. That reasoning *a priori*, we have no just grounds for concluding that the properties generated in any chemical union should arise from both or all its constituents, rather than either separately, but that they *may* arise in either way according to circumstances; and further that a careful examination of the phenomena attendant on chemical union must decide whether they *do* arise both ways, according to circumstances.

3d. That an attentive examination of chemical combinations, as completely demonstrates that they *do* arise in both ways according to circumstances, as the reasoning *a priori*, that they *may*.

4th. That the belief in the reciprocity of chemical action, is not inconsistent with the admission of the two last propositions.

5th. That those who believe in but three elementary substances, and that hydrogen is the base of the muriatic acid, must necessarily admit that the reasonings derived from the consideration of the binary acid compounds, in proof of oxygen being the principle of acidity, are as decisive and certain, as those in any proposition in Euclid, inasmuch as they are as complete as the nature of their belief will admit.

6th. That those who do not believe that hydrogen is a constituent, either in all metals or in phosphorus, sulphur or carbon, may render the demonstration even more complete (if this indeed be possible) only by applying the rule to every one of the accession of binary acid compounds, which a disbelief in that extensive agency of hydrogen would necessarily produce: among this accession would be the five metallic acids, together with the sulphuric phosphoric and carbonic acids.

7th. That those who believe in this extensive range of hydrogen, but not in it, as the base of the muriatic acid, must admit, in case it should be clearly demonstrated that it was the base of that acid, that the arguments which have been adduced to prove oxygen the principle of acidity are conclusive.

8th. That the only method of proceeding to attain a knowledge, of the principle of inflam-

mability, is by treating the subject in an analogous manner to that by which we made evident the principle of acidity.

And we hope to prove in a future communication that the supposition of any substance, being a principle of inflammability, *i.e.* an only body which will leave its caloric and light in part or entirely, to unite with other bodies, is only tenable upon an hypothesis, which will not be readily admitted; and even then, we may add, the proof is not quite complete, but as we said before, we beg leave to lay before the gentlemen of the society our peculiar opinions on that head in a future essay.

On the prognostic signs of the weather. By James Cutbush. *Professor of chemistry, &c. in St. John's College, Philadelphia.*

Meteorology, a branch of natural philosophy, is important in many respects. It interests mankind as it furnishes predictions of the change of weather. Within a few years, many philosophers have turned their attention to this subject, since which, various observations have been made. The experiments and remarks of De Luc, De Saussure, Jones, Marshall and Kirwan afford a number of facts in meteorology. Observations have been made by these and other gentlemen, on different signs, which arise from a combination of circumstances, in order to establish some general rules respecting the changes of the weather. Thus we have animate and inanimate substances, as well as the barometer, thermometer, hygrometer and electrometer, as active instruments to accomplish this end. If these, as well as the action of winds, answer in their results, a correct datum is at once established. Although inaccuracies may arise from a multiplicity of circumstances, and events may occur contrary to expectations, yet upon the whole, when these instruments and signs are properly used, they must eventually lead to truth, the grand *desideratum* in philosophical inquiries.

As well as a correct application of the meteorological instruments and bodies, as we have already remarked, the various states of the sky, the clouds,

the changes in the wind, &c. are essential in this investigation.

1. *Prognostics by the spider, &c.*....Mr. Disjonval, an adjutant-general in the Dutch service, while a prisoner, made a variety of experiments and observations on the spider, which have shown that they may be employed with advantage in meteorological inquiries. His remark is, "that spiders are particularly excellent as prognosticators of changes in the weather, being more certain than the barometer, giving their indications a longer time before hand, and having this advantage to the lower class of people, that they cost nothing." On the common house spider he made the following remarks. Against fine weather, it peeps out its head, and stretches its legs out of its hole; this the further the longer the fine weather will continue. Against bad weather, it retires farther back; and against very tempestuous weather it turns quite round, showing nothing but its hinder parts to the observer, thus acquainting him with the approaching change of the weather. At the commencement of fine weather, the webb, with which it surrounds its corner, is but of moderate extent; if the fine weather will be lasting, it enlarges it to two or three inches; and if it do this several times repeatedly, we may be certain, that the weather will continue fine for some time.

On the 22d of July 1795 Mr. D. foretold, from the behaviour of his spiders, a fortnight before hand, that the water of the Rhine would fall so

as to render it passable by a bridge of boats; and in this manner it was always passed.

His observations on the general conduct of the spiders in the winter season, are also important. They are also prognosticators of approaching cold. If frost and snow be coming on, they either seize upon the webs already made, in which case obstinate battles frequently ensue, or they make new ones, and labour diligently at them.

Disjonval found, from several attentive observations, that, from the first of the spiders putting themselves in motion to the setting in of the frost, nine days generally elapsed. We have a striking instance of the justice of this observation in the beginning of February 1793. The weather was fine, warm, and there was no symptom of approaching frost. It might have been supposed that fires would be no longer required; but on the 4th of February Mr. Disjonval announced, that a great alteration in the weather would ensue, as, besides other remarks of a similar kind, he had seen three spiders webs one over another, in a place where there was not one the preceding evening. On the 9th of February there was ice, and by the 13th all the canals were frozen over. It was now probable, that with the breaking up of the frost the winter would terminate. This was the opinion of Mr. Disjonval himself: and he felt no small satisfaction, in having been able to foretell the freezing of the canals to a whole town, when such a circumstance was least expected. A complete thaw in fact came on; but on a sudden he observed, con-

trary to all expectation, a general bustle among his spiders on the last day of February. They ran backwards and forwards, began to spin webs diligently, and attacked one another. Hence he inferred, that some remarkable change was taking place; and that very dry weather at least, if not very cold would ensue. This conjecture he announced to the principal bookseller in the town, and through him to the public. Two days after it rained, which seemed no way favourable to his prognostication; and this rain continued for five days, so that the validity of his prediction appeared daily more questionable. Still, however, attentive to the proceedings of his spiders, he wrote every day to the same bookseller, telling him he continued firm in the persuasion of the approach of cold and dry weather. On the 8th of March it blew hard; on the 9th it snowed; and on the 10th the frost was so sharp, that all the canals were frozen over again.

The greatest and most striking instance of the importance of these observations, and the dependence that may be placed on predictions respecting the weather from them, is the conquest of Holland by the French in the winter of 1794-5. Disjonval's keeper was inclined to the patriotic party, and in consquence treated his prisoner with less strictness. Through his means Disjonval gave notice to the patriots, that a hard Winter would ensue, which would render all the rivers and canals passable on the ice. The taking of the town by the French afforded him the only hopes of be-

ing emancipated from his long imprisonment: it may be supposed, therefore, that he observed his spiders with the utmost care and attention. In the beginning of December he heard, to his great alarm, that the people talked of a capitulation, which would have annihilated his hopes at once. He used every means in his power, to make known, that, from the operation of his spiders, a very severe frost would inevitably come on, and this within a fortnight at farthest. The people gave credit to his prediction, did not capitulate, and on the 29th of December, the frost was so hard, that the French were able to pass the wall. The aristocratic party flattered themselves, notwithstanding, that the frost would soon break up, as on the 12th of January the water rose, and was turbid, which was considered as a certain indication of a thaw. Disjonval in the mean time, wrote from his prison to the editor of the Utrecht gazette, that, before three days had elapsed, a more severe cold than the former would take place. On this occasion the spiders proved incomparably better prophets than the turbid water; on the 14th of January the wind rose, on the 15th it froze, and on the 16th the French entered Utrecht; thus the prisoner regained his liberty.

He continued carefully to observe the spiders he could find, in order to give the French general fresh information, which was of such importance to him in this daring enterprize.

On the 20th of January a sudden thaw came on. The general was alarmed for the fate of an army

of an hundred thousand men, with a train of artillery, and began to think of a speedy retreat. But Disjonval had recourse to his spiders, and they foretold frost. He sent a couple of these little prophets to the French general: they were credited, their prophesies were fulfilled, and the French conquered Holland.

Such a striking circumstance induced the French executive directory to institute an inquiry into this branch of knowledge; observations will be made, most probably in other countries; and these *odious* spiders may be found of the greatest importance, in meteorology, and consequently of great use in common life.

Besides what we have already said on the subject of the spiders, the leech has been found to answer the same purpose; namely, its rising or falling in the bottle as indications of particular changes; but how far observations drawn from this circumstance are true, remains to be decided. Amongst the general or common pragnostics of the weather, we may reckon such as are derived from birds, beasts, insects, reptiles, and plants, to which might be added great part of the wood work in houses, as doors, windows, window shutters, &c.

Birds in general retain in the quill part of their feathers, a quantity of oil, which, when they feel an extraordinary degree of moisture in the atmosphere, they express by means of their bills, and distribute it over their feathers to secure their bodies against the effects of an approaching shower.

Swallows, in pursuit of flies and insects, on which they prey, keep near the earth in wet weather, and in dry weather from the same cause, they fly much higher.

Domestic animals, as cows and sheep, but particularly the latter, on the approach of rain, feed with great avidity in the open field, and retire near the trees and hedges as soon as they are satisfied. In fine weather, they graze and lounge about eating and resting alternately, with apparent indifference.

The pimpernal, commonly called the peep-a-day, or shepherds weather glass, closes its leves before rain; and the down of the dandelion is much affected by moisture.

All wood, even the hardest and most solid, swells in moist weather, the vapours insinuating themselves into the pores of the trees, and also into the wood work of houses.

Insects and reptiles of all kinds, seek or avoid rain, according to their respective habits, by these means giving notice of every change of weather.

Dr. Manners informs me, that when the tree frogs are observed to croak unusually, and the swine to prepare for themselves beds, by carrying straw and and other articles in their mouths, rain and stormy weather is always to be apprehended. This effect, Dr. Manners has often witnessed. There are a number of signs, in use among the peasantry, arising from the different habits of animals, that are certain prognostics of the weather.

Sect. 2. *Prognostics by the Barometer.*

Many experiments and observations have been made with this instrument, not only as connected with meteorology, but also in a mathematical view, as in ascertaining the heighth of mountains, &c.

The variations in the barometer in different climates, as for instance, as we go to, or recede from the equator, have occasioned some inaccuracies on the subject. The countries, however, that are situated about the equator, are subject to the changes of the weather, though it is more constant there, than in the temperate climates: there are changes there of humidity and dryness, rains and fair weather, storms and tempests, much more violent than with us.

Mr. Dalton has given the following general rules and observations, for judging of the weather.

1. The barometer is the highest of all during a long frost, and generally rises with a N. E. wind; it is lowest during a thaw following a long frost, and is often brought down by a S. W. wind.

2. When nearest the high extreme for the season of the year, there is very little probability of immediate rain.

3. When the barometer is low for the season, there is seldom a great night of rain, though a fair day in such a case is rare. The general tenor of the weather at such times is short, heavy, and sudden showers, with squalls of wind from the S. W. the W. or N. W.

4. In summer, after a long continuance of fair weather, with the barometer high, it often falls gradually, and for one, two, or more days, before there is much appearance of rain. If the fall be sudden and great for the season, it will probably be followed by thunder.

5. When the appearances of the sky are very promising for fine weather, and the barometer, at the same time low, it may be depended upon that the appearances will not remain such long. On these occasions, the face of the sky changes very suddenly.

6. Very dark and dense clounds pass over, when the barometer is high, without rain; but when the barometer is low, it sometimes rains almost without any appearance of clouds.

7. All appearances being the same, the higher the barometer is, the greater is the probability of fair weather.

8. Thunder is generally preceeded by hot weather, and followed by cold and showery weather.

9. A sudden and extreme change of the temperature of the atmosphere, either from heat to cold, or cold to heat, is generally followed by rain within 24 hours.

10. In winter, or during a frost, if it begin to snow, the temperature of the air generally rises to 32 degrees, and continues there while the snow falls; after which, if the weather clear up, expect a severe cold.

11. The aurora borealis is a prognostic of fair weather.

The changes which take place in the atmosphere are principally marked by the rising and falling of the barometer, which apparently is caused by heat and cold, the hands with which nature performs her meteorological operations: by the former the atmosphere is rarified, and consequently becomes light; by the latter it is condenced and consequently becomes heavy. Hence probably the old remark, that a storm generally follows a calm; for during a calm the air is rarified and expanded, and the cold air will rush forward in a strong current to restore the equilibrium, and necessarily produce what is called a gale of wind, the violence of which also will be in proportion to the degree of the preceeding rarefaction of the atmosphere.

For these reasons, the barometer falls suddenly whilst the air is expanded before a gale of wind, and rises again gradually as the condensed air returns, and the gale in like manner by degrees subsides.

It must, however, be observed, that an extraordinary fall of the mercury will sometimes take place in summer, previous to heavy showers of rain, particularly if attended with heavy thunder, and lightning; but in spring, autumn and winter, the sudden descent of the barometer indicates principally violent wind. Upon these principles likewise we may account for the rise and fall of the barometer in different zones. In the torrid zone, particularly at St. Helena, and the Islands of the Pacific ocean, it seldom varies more than three

tenths; at Madras about five tenths; in the south of Europe not more than an inch and two tenths; in England it varies two inches and a half; in America about the same; and in Petersburg three inches four tenths.

The following observations of Mr. Patrick, on the application and use of the barometer, in meteorological inquiries, may be found useful:

1. The rising of the mercury presages, in general, fair weather; and its falling foul weather, as rain, snow, high winds, and storms.

2. In very hot weather, the fall of the mercury indicates thunder.

3. In winter the rising presages frost; and in frosty weather, if the mercury falls three or four divisions, there will certainly follow a thaw, but in a coutinued frost, if the mercury rises, it will certainly snow.

4. When foul weather happens soon after the falling of the mercury, expect but little of it, and on the contrary expect but little fair weather, when it proves fair shortly after the mercury has risen.

5. In foul weather, when the mercury rises much and high, and so continues for two or three days before the foul weather is quite over, then expect a continuance of fair weather to follow.

6. In fair weather when the mercury falls much and low, and thus continues two or three days before the rain comes, then expect a great deal of wet, and probably high winds.

7. The unsettled motion of the mercury denotes uncertain and changeable weather.

But to these remarks may be added, that, when the barometer suddenly falls two or three tenths, without any material alteration in the thermometer, and the hygrometer is not much turned towards moist, a violent gale of wind may be expected.

When the hygrometer inclines towards moist, with only a trifling wind; and when the barometer falls considerably, and the hygrometer turns much towards moist, the thermometer remaining stationary, and rather inclined to rise than fall, both violent wind and rain are likely to follow in the course of a few hours.

Owing to a misapplication of the barometer, and frequently from an imperfection in the instrument itself, errors may frequently occur. According to the following rules, several of these objections may be obviated. It will then instruct the farmer, with certainty when to hasten and when to delay his work; it will direct the mariner when to prepare against the storm, or the tempest, and inform him when he may take his rest with security: it will teach the traveller how to clothe himself for his journey. In short, it will not only discover the approach of rain, and the return of fair weather, but will also enable the observer to determine when either will be of long or short continuance.

1st. In making your observations on the barometer, pay no regard to the words engraved on the plate, but fix your attention on the moving index, which you will do right to regulate very frequently.

2d. The mercury is always lowest in fine weather, especially if accompanied with rain.

3d. It is always highest when the wind is at east or north east, particularly if not strong.

4th. It is high in calm frosty weather.

5th. If it sink in frosty weather, a thaw of snow always follows.

6th. It sinks during violent winds.

7th. When the mercury rises fair weather is approaching.

8th. When it sinks, foul weather may be expected.

9th. When the weather suddenly changes from foul to fair, upon the first rise of the mercury, or from foul to fair, upon its first sinking, such weather will not continue long.

10th. If in wet weather the mercury gradually rises far about two days before the change, a continuance of fair weather may be expected.

11th. If it gradually sinks for about five days during fair weather, a continuance of wet generally follows.

12th. When the mercury is stationary, whether high or low, a continuance of the weather, such as it then is, may be expected.

13th. If you are desirous of ascertaining the weather for a day, or a few hours, it will be necessary to inspect very accurately the surface of the mercury; if it be remarkably convex or round, it affords a strong presumption of fair weather. If, on the contrary, it be very concave, or hollow, it implies almost immediate rain.

Sect. iii. *Prognostications with the thermometer either alone or with other instruments.*

The thermometer is important as a meteorological instrument for various reasons. Its use is to ascertain the variations of temperature. While this instrument is so employed, the hygrometer is used to ascertain the quantity of moisture, and the electrometer to shew that of electricity which prevails in the atmosphere.

From the thermometer we can ascertain, in winter, when the cold diminishes rapidly, the coming of rain; and in summer, by a sudden rise of the mercury the same result.

If the air in foggy weather becomes hotter by the action of the suns rays, the fog generally dissipates, and the air remains serene: but if the barometer fall, and the change of temperature (indicated by the thermometer) be from a south or a south west wind, the fog rises and forms itself into clouds, and its ascension is generally a sign of rain.

When the barometer remains high and stationary, the natural and factitious hygrometers indicate dry air, the canopy of the sky lofty, and the wind north easterly, we may uniformly expect settled fair weather; while a light and moist atmosphere, the canopy of the sky low, and a south west wind, certainly portend a wet season.

Sect. 4. *Indications of the weather by the clouds.*

A rain bow in the morning is said to be the shepherds warning, but a rain bow at night is the shepherds delight. This principle may be correct on philosophical principles: for if the clouds to the westward in the morning are saturated with moisture, which they must be to produce a rain bow, these clouds proceeding from W. towards E. may bring rain; whereas, on the contrary, when the sun sets perfectly clear, and the clouds to the eastward are moist, it is a proof that the wet clouds are past, with a westerly wind, and the shepherd therefore, may reasonably expect fine weather on the following day.

When it rains with an east wind, it probably will rain 24 hours. The weather generally clears at noon, but when it rains at mid-day, it seldom clears again till sun set.

The air, when dry and warm, continues to abate and retain the moisture continually evaporated from the earth; as therefore the sun advances towards the meridian, and for an hour or two afterwards, he dries and warms the air; and consequently the rain is likely to cease at that time. But if there should be so much water in the atmosphere, that the heat of the sun is not sufficient to produce these effects, in that case the rain will probably continue some hours longer. From the appearance of the clouds, the kind of weather to be expected may be

known. The following facts may, therefore, prove useful:

When the clouds are formed like fleeces deep and dense towards the middle, and very white edges, with a bright blue sky about them, they generally soon fall in hail, snow, or in hasty showers of rain.

There is no sign of rain more certain than two different currents of clouds, especially if the undermost fly fast before the wind; when this happens in summer, there is seldom wind at the time, and thunder generally follows. In winter the light vapor, or scud as the sailors call it, often comes rapidly against the wind, and a gale is soon after to be expected.

The transparency of the air is to the inhabitants of the Alps one of the most certain signs of rain; when the distant objects appear distinct and well defined, when the sky is of a deep blue, they consider rain as near at hand, though no other sign appears. I have been informed by a gentleman, to whom I am under obligations for other observations, that this sign, from the transparency of the air is by no means local, but is often observed, that in such a state of the air, the sailors say the land, or some other object looms near, and expect bad weather.

When the sky, in a rainy season, is tinged with a sea-green colour, particularly near the horizon, when it ought to be blue, the rain will continue and increase. If it be of a deep dead blue, it will

be showery: this is more particularly found to hold true near the sea coast.

Clouds of a similar appearance produce thunder in summer, and snow in winter; such clouds are broken, and irregular shaped, heaped one on another, and from their uncommon density project towards the earth.

After a thunder storm, when it has been of considerable duration, the wind generally, if not always, veers to the quarter from whence the first clap proceeded.

A close sultry day, the current of air scarcely perceptible, is often succeeded by a change of wind.

The wind shifting from point to point round the compass,generally denotes rain. If after a continued rain from a muddy sky, the horizon appear lighter in any quarter, expect the wind from that quarter.

Some further remarks respecting the winds, may be interesting in a meteorological view.

Violent winds generally abate towards sun set; for, if we admit that wind is only a current of air put in motion by the rarefaction of the atmosphere in some particular place, and that this current of air is moving towards the rarefaction to restore the equilibrium, we must suppose, that as the sun declines the rarefaction will diminish, and consequently the velocity of the wind decrease. But this observation, in my opinion, rather applies to the temperate than to the torrid zone; for in

whirlwinds and hurricanes the contrary may often occur.

When the wind follows the course of the sun, it is generally attended with fair weather. This frequent and regular change of the wind, which is never more than a moderate breeze, proves that there is no point of considerable rarefaetion near, and therefore, the current of air follows immediately the sun's course: it always happens in summer, but very seldom when the sun's meridian altitude is less than 40 degrees.

It is a well known fact, that before rain, particuly in summer, a strong smell is perceived from drains and common sewers, as well as from every other body, emitting a great quantity of effluvia. During fair weather, even in the summer, the atmosphere readily absorbs all the vapours and exhalations from the earth until it is completely saturated, and consequently the effluvia from the bodies which emit them, will then be confined and ascend in a narrow compass, like the smoke of a chimney in dry weather, almost perpendicularly; but when the air is saturated with moisture, and becomes rarefied and expanded, as it always does before rain, the volume of air containing the effluvia will be extended horizontally, and diverge from these different bodies as from a centre, and will be sensibly perceived on all sides, but will of course be most perceptible on that to which the current of air or wind moves.

Sect. 5. *Of the instruments used to ascertain moisture in the atmosphere.*

The *hygrometer* is an instrument intended to discover the moisture contained in the atmosphere. The substances affected by moisture are numerous: thus wood expands by moisture and contracts by dryness; on the contrary, cord, catgut, &c. contract by moisture and lengthen by dryness, consequently, the contraction and expansion of these bodies indicate different states of the air with respect to moisture. The twisted beard of a wild oat, with a small index fixed to it, moveable against a scale makes a very good hygrometer. Mr. De Luc has shown that whale bone and box, cut across this fibre, answers extremely well.

Hygroscopic substances are of three distinct kinds. 1st. Those that seize on the water of vapour by an affinity for that liquid; among these are acids, alkalies (all deliquescent salts) and and some earthy substances. 2d. Those that imbibe the water, by the tendency it has to propagate itself in capillary pores, but from their nature receive no sensible increase of bulk by its introduction; such as porous stones. 3d. Those that, imbibing a certain quantity of water, are thereby expanded, such as most of the vegetable and animal substances. M. De Luc, by a long series of experiments, to which I must refer you, shows, that the substances of the last class are the only

ones proper for hygrometers. From time immemorial, the effects of moisture have been considered as prognostics of the weather, as is evident by the confidence the housewife places in her salt box, the carter in his whip leather thong, and the sailor in his shrouds.

The application of the hygroscopic substances already mentioned, to the purposes of meteorological observation, is affected by various contrivances, some of which are extremely simple, and others again complicated. The *weather gage*, as it is termed, sold by the toy sellers, although not intended to tell the degree of moisture, as is the true hygrometer, is at once an instrument of utility and elegance.

Sect. 4. *Of the instrument used to discover electricity in the air.*

With respect to atmospherical electricity, as connected with this subject, little need be said. From experiments made with Mr. Bennet's electrometer, it appears, that particular states of the atmosphere, influence certain changes so considerably, that electricity would seem to be a primary agent in many of these changes. In stormy weather we see the electricity strong, then null, and in a moment after arise to its former state: one instant, vitreous; the next, resinous. M. de Sausure says, that he has seen these changes succeed with such rapidity, that he had not time to note

them down. When rain falls without a storm, these changes are not so sudden; they are however, very irregular. Rain or snow almost uniformly gives vitreous electricity. Foggy weather produces the greater quantity of electricity.

Experiments, therefore, with the electrometer added to those of the barometer, thermometer, and hygrometer will afford correct deductions.

EXPERIMENTS AND OBSERVATIONS

On the effect of light on vegetables and upon the physiology of leaves, By JOHN MANNERS, M.D. *of Philadelphia. Honorary member of the Medical, Medical Lyceum, and Columbian Chemical Societies of Philadelphia; Member of the Philadelphia Agricultural, and Linnean Societies, Fellow of the Academy of Natural Sciences. &c.*

AFTER the memorable discovery of oxygen gas or de phlogisticated air, *that sine qua non of combustion, acidification and respiration*, by the celebrated Dr. Priestley on the 1st. of August 1774, various methods were discovered by which oxygen gas might be obtained.

Dr. Priestley obtained it by exposing the black oxyde of Manganese *per se* in an iron retort to an obsure red heat and receiving the gas over the Pneumatico-chemical apparatus, of which he was the inventor. Though Dr. Mayow first invented the method of collecting gasses over water, which was afterwards improved by Dr. Hales, Dr. Brownrigg and Mr. Cavendish.

This was the method of obtaining oxygen gas preferred by Dr. Woodhouse.

Scheele, who procured it shortly after Dr. Priestley, added to the black oxide, sulphuric acid, which renders so high a degree of heat unnecessary. By which a glass, may be substituted for an iron retort. 1400 cubic inches of oxygen gas may be obtained from one pound of the black oxyde; but always contaminated with a small portion of carbonic acid and nitrogen gas. It may be freed from the former by lime water, but from the latter it cannot be freed by any known process.

The oxydes of lead, particularly *minium and litharge*, afford it abundantly when either exposed to heat, *per se* or with the addition of sulphuric acid. But like that obtained from manganese mixed with carbonic acid and azote or (as M. Chaptal has more properly called it) nitrogen.

The oxydes of mercury, particularly the *red precipitate per se*, afford it in large quantity. But as M. Chaptal has proved holding mercury in solution, as he found the oxygen obtained from mercury when inhaled into the lungs for medical purposes, excite *Ptyalism*. He also, by exposing it to extreme condensation by cold, procured a mercurial oxyde. According to him, every ounce of mercury yields one pint of oxygen gas.

In fact almost all the oxydes of metals (except the alkaline and earthly oxydes lately discovered by Sir H. Davy) may be so treated as to afford oxygen.

The nitrate of potash when exposed to heat in a retort affords oxygen gas nearly in a state of purity containing only a minute portion of azote. One

pound of this salt is said to yield 1200 cubic inches.

The oxymuriate of Potash affords it in large quantity, and in a state of great purity.

Very pure oxygen gas may be obtained from the large extremity of the egg. For an account of which see Dr. Coxe's Inaugural Dissertation.

Different acids, when exposed to light emit oxygen, as the *nitric*, *oxymuriatic*, &c. see Mrs. Fulhame on combustion.

Different metallic salts as *muriate of silver*, *nitrat of silver*, *muriate of gold*, *muriate of platina*, &c. when exposed to the rays of the sun become reduced and, as M. Berthollet first observed, emit oxygen gas. And this effect is not produced equally by all the different coloured rays. After Sir Isaac Newton decomposed the rays of light with the prism into the seven primary colours, red, orange, yellow, green, blue, indigo and violet, Dr. Hooke demonstrated by those simple and well known experiments of placing different coloured pieces of cloth in the snow and marking the different degrees they sank, that the different coloured rays, possessed different heating powers. These experiments were afterwards repeated by Dr. Franklin, and still more lately in a more simple way by Mr. Davy. Bnt the philosophic world are chiefly indebted to Dr. Herschell, whose experiments were afterwards repeated and confirmed by sir Henry Englefield, for a complete division of the solar spectrum in to *calorific* and *colorific* rays. Scheele discovered that the reduction of metals takes place more

quickly in the violet ray. And Dr. Wollaston, Richter, and Bockmann investigated the subject still further and proved that the reduction is even more rapid beyond the violet extremity of the prismatic spectrum and consequently discovered deoxydizing rays.

The abbe Fontana found certain insects when exposed to the rays of the sun emit oxygen. And the green matter which Dr .Priestley placed among the conferva, which Mr. Senneber supposed the *conferver cespitosa filis rectis undique divergentibus Halleri*, and which Dr. Ingenhousz supposed a mass of animalcula, when exposed to the sun afford vital air in large quantity.

This fact was first discovered by Dr. Priestley as we are informed by Dr. Darwin in the follow-lines :

Sylphs ! you, retiring to sequester'd bowers,
Where oft your *Priestley* woos your airy powers,
On noiseless step or quivering pinion glide,
As sits the Sage with Science by his side ;
To his charm'd eye in gay undress appear,
Or pour your secrets on his raptured ear,
How nitrous Gas, from iron ingots driven,
Drinks with red lips the purest breath of heaven ;
How wile Confervera, from its tender hair,
Gives in bright bubbles empyrean air,
The crystal floods phlogistic ores calcine,
And the pure ether marries with the Mine.

Botanic Garden, Canto 4, 179.

It would be foreign to the present subject to enter particularly into the natural history of these confervæ, or the interesting speculations of Dr. Priestley, Dr. Girtanner, Dr. Ingenhousz, Dr. Darwin, Mr. Buffon, my father-in-law professor Cooper, and other philosophers respecting their origin, and the theory of equivocal generation, or the doctrine of organic particles. But certainly the ingenious and numerous experiments of Mr. Buffon (Nat. Hist. v. 2d. p. 148, sect. 1. 8vo. Ed.) and the interesting experiments of Reaumur, Ingenhousz, Ellis and other naturalists have never been refuted, and deserve much more of the attention of philosophers than they receive at the present day. As the production of mushrooms from horse dung, the conferva from stagnant water, microscopic animals from veal broth boiled in a vessel hermetically sealled, &c. can only be satisfactorily accounted for by the doctrine of organic particles. Dr. Darwin the philosopher and poet has expressed his opinion on this subject with equal poetic grandeur and philosophic elegance.

Thus the tall Oak, the giant of the wood,
Which bears Britannia's thunders on the flood ;
The Whale, unmeasured monster of the main,
The lordly Lion, monarch of the plain,
The Eagle, soaring in the realms of air,
Whose eye undazzled drinks the solar glare,
Imperious Man, who rules the bestial croud,
Of language, reason and reflection proud,
With brow erect, who scorns this earthy sod,
And styles himself, the image of his God ;

Arose from rudiments of form and sense,
An embryon point, or microscopic Ens!
Temple of Nature, Canto 1. 303.

Dr. Priestley, Dr. Ingenhousz and Sennebier discovered nearlyabout the same time that the green leaves of vegetables when exposed in a glass vessel filled with, and inverted over water emit oxygen gas, and this fact is detailed by every author who has written professedly on either chemistry, or vegetable physiology.

Various explanations of the rationale of the phenomena attending it have been given by different philosophers.

It has generally been supposed that the water is decomposed, that the oxygen of the water unites with the heat and light of the rays of the sun, while the Hydrogen of the water, unites with the vegetable leaves, for their support. But the scientific Dr. Woodhouse in a course of well conducted experiments proved that the water is in no instance decomposed by the vegetable leaves, and that the oxygen gas is the product of the decomposition of carbonic acid with which the water is impregnated. I have repeated, multiplied and varied the experiments of our respected professor, and satisfied myself of their correctness. I shall therefore mention some few of those which I think most conclusive, as to enumerate the whole would be tedious and not interesting. I put some leaves in a vessel filled with, and inverted over water; gas began immediately to ascend. I took

out the same leaves and put them in lime water, but not a particle of gas was obtained in this experiment. I took out the leaves, washed them and put them in water, and gas was immediately disengaged from them.

But lest the lime water either, by its stimulus or chemical properties, might destroy the power of the leaves to decompose water, I placed them in distilled water, and in water which had been boiled two or three hours: but in neither of these experiments did I obtain any gas. I then impregnated water slightly with carbonic acid, and placed leaves in it, and now gas was procured more abundantly than in any of my former experiments.*

How wise the economy of nature, in thus forming the animal and vegetable kingdoms reciprocally and essentially to serve each other! And how beautifully does it exemplify the harmony of the material world! That while the animal kingdom by respiration consumes oxygen gas and converts it into carbonic acid, (not to mention the immense quantity of oxygen converted into fixed air by combustion,) the vegetable kingdom from carbonic acid generated by respiration, combustion, fermentation and all its numerous sources, by the agency of light manufactures the vast mass of atmospheric oxygen, and thus restores the equilibrium of nature.

* See some interesting remarks on this subject, by Professor Cutbush, in his Philosophy of Experimental Chemistry, vol. I

" When Morn escorted by the dancing Hours,
O'er the bright plains her dewy lustre showers,
Till from her sable chariot Eve serene.
Drops the dark curtain o'er the brilliant scene,
You form with chemic hands, the airy surge,
Mix with broad vans, with shadowy trident urge.
Sylphs! from each sun-bright leaf, that twinkling shakes,
O'er Earth's green lap, or shoots amid her lakes,
Your playful bands with simpering lips invite,
And wed the enamour'd Oxygen to Light.
Round their white necks with fingers interwove,
Cling the fond Pair with unabating love.
Hand link'd in hand on buoyant step they rise,
And soar and glisten in unclouded skies.
Whence in bright floods the *Vital Air* expands,
And with concentric spheres involves the lands;
Pervades the swarming seas and heaving earths,
Where teeming Nature broods her myriad births;
Fills the fine lungs of all that *breathe* or *bud*,
Warms the new heart and dyes the gushing blood:
With Life's first spark inspires the organic frame,
And as it wastes renews the subtile flame.

Darwin's Botanic Garden, Canto iv. 25.

In repeating these experiments I observed a phenomenon which I had no where seen mentioned in either botanical or chemical authors. I perceived all the gas to be disengaged from the inferior surface *(pagina inferior)* of the leaf. This surprized me as I was better prepared to meet a contrary result: Dr. Darwin tells us in his *Phytologia* that the su-

perior surface of the leaf, performs the pulmonary function and that if this surface of the leaf be covered with a coat of oil or varnish it dies. The experiments of M. Bonnet would seem to prove the same. And Mr. Parke in his chemical catechism expressly says the gas is only disengaged from the upper surface of the leaf. I mentioned the occurrence to Dr. Barton whose extensive knowledge of botany, as well as chemistry, would enable him to give me more satisfactory information on the subject. But he was no less surprised than myself.

I was therefore induced to repeat my experiments; but always with the same result. I took two leaves of the *prunus cerasus*, or common cherry, which were the leaves with which I had made all my former experiments and placed the superior disk of the one, and the inferior disk of the other leaf under water in an inverted bell glass to the direct rays of the sun; but the result was precisely the same. These experiments were seen by Dr. Barton. I then repeated the experiments of M. Bonnet and Dr. Darwin by coating different surfaces of different leaves, but with a different result from those philosophers. All those leaves whose upper surfaces were coated with oil, remained healthy and vigorous; while those whose under surfaces were coated, died in two or three days. These specimens I had also the pleasure of showing to Dr. Barton who

requested I would pursue the subject further, and was so kind as to give me access to any of the botanical specimens in his possession, I gladly availed myself of his liberality. I procured from him the leaves of a variety of different vegetables and exposed them in water to the rays of the sun. But here I found a difference of result, some emitting the gas from both surfaces, while others only from the inferior page. I took two leaves of the *prunus lauro cerasus* and exposed them in different vessels filled with water; the one with the upper, the other with the under surface to the sun; but in both the gas was disengaged from the inferior page. I took two other leaves of the same vegetable and coated the upper surface of the one, and the under surface of the other. The result was, that the one whose upper surface was coated remained healthy and vigorous after the other was dead. I have repeated and varied these experiments with the leaves of some species of *Magnolia*, *Peach*, and the leaves of a variety of *trees* with the same result. But I have repeated them with a considerable number of the leaves of *herbaceous plants* and here in every instance with a different result, the gas being disengaged either indiscriminately from both surfaces, or else chiefly from the upper. May not this explain the difference of result between my experiments and those of M. Bonnet and Dr. Darwin? probably they experimented with the leaves of herbaceous plants only, while I

experimented with leaves of both trees and herbaceous plants.

Upon the whole I am warranted to conclude, as far as my experiments go, (though I acknowledge they are not very conclusive) that the leaves of trees and herbaceous plants perform the same function with different surfaces of their leaves.

SPECULATIONS ON LIME.

By Joel B. Sutherland, M. D. *Honorary member of the Columbian Chemical Society, and Fellow of the Academy of Natural Sciences.*

TO enumerate all the advantages, or disadvantages to be derived from lime, is not my present object; but only to call your attention, to circumstances relative to lime, which are novel, and I hope not of an uninteresting nature. Lime for ages past, has been made use of in the formation of mortar, a cement of great importance to the world; but although this cement is of such considerable importance to the world at large, yet, in certain instances, I think it becomes highly prejudicial, not only to our houses, but to our constitutions, and that altogether on account of its incorrect formation.

In general, pure sand loom, water and lime constitutes this valuable cement: and when this is the case, no disadvantages can arise from its use.

But when lime is brought to this city which is impregnated with the muriate of soda and substituted in place of that which contains little or, none of that ingredient, the reverse is the exact statement of the case; for the muriate of soda combined with the sand when intimately mixed with the other ingredients in mortar, gives so much coldness to the mass, that during the whole summer, the vapour, which floats in the atmosphere, is almost incessantly precipitated upon the wall. In confirmation of this I shall offer bnt a single fact to your consideration, which I have no doubt will substantiate what I have asserted. A gentleman with whom I am very intimate, found it necessary some years ago, to plaister anew a portion of his house, on account of the innovation made with respect to the regulations of the street in which he resided.

In this case, the gentleman instead of procuring the purest sand, purchased *that* species containing common salt, suspecting this was the best on account of its whiteish appearance. But from that time up to the present period, that portion of the wall formed by the muriated sand, has been profusely sweating, (to use a common phrase) while the old part of the wall produces little, or no fluid, on account of its inferior temperature.

From what has already been said, it must be very evident to a man of the narrowest mind, that in choice of sand; that which contains the smallest quantity of salt is most assuredly the best. But before I come to a conclusion on this part of my

subject, I shall, offer a single conjecture to the Society. What would be the effect, if we should plaister our cellars *in toto* with a mortar containing a very large quantity of common salt, might we not expect to obtain from them, all the advantages of a *spring house.* I shall now proceed to say a few words upon another subject, in which lime is called into question. Hens fed from the hand of a citizen of any populous town, not unfrequently deposit their eggs, without a calcareous covering. And why? The people in general explain the fact, by saying fat hens sometimes lay their eggs without shells. This may satisfy the vulgar and incurious mind; but one of a philosophic nature, endeavours to explain it in a more rational manner. In a city like this chickens do not obtain a Quantum sufficit of substances containing lime to form the shell. This then is the plain and obvious reason, why such a phenomenon, so often takes place among the feathered race. If we should feed our fowls with more calcareous substances, and less of the luxuries of the city, we would seldom, or, perhaps never witness such occurrences. I have no doubt if we were acquainted with persons possessing soil, containing a large quantity of lime, we would find that all the eggs laid in that neighbourhood, would have a stronger covering than elsewhere. If lime then, should thus act upon the constitution of the hen, so as to form the shell of the egg; would it not be Philosophic (nay I will go so far as to say correct) practice, to prescribe substances contain-

ing lime in cases of the fracture of bones. But in this instance, as bone differs from the shell with respect to the quantity of its ingredients, and perhaps I might have said its quality; for the one contains little or no Phosphoric acid, while the other contains a considerable quantity, we should advise the use of the phosphate of lime, instead of the carbonate or any of the other cretaceous substance without the use of phosphate at the same time.

Now all are convinced that bone must form in a smaller space of time in proportion as the materials are in the human system which exist in every bony substance, of course then the novel method I have recommended would produce a union in the fractured extremities of the bone much sooner, than where no preparations of lime had been administered. Indeed I should not be astonished to hear, that a bone which required six or eight weeks to become firm in the old way, would, according to the practice just recommended, become firm in half the time.

But, here some may say my expectations are too sanguine, for in Rickets, there is a decomposition of the bone, and yet calcareous materials seem to produce no effect. Be it so. Yet there is a very great difference between the simple fracture of a bone, and a morbid predisposition in the whole bony substance. I hope then you are all convinced, that because lime does not produce any good effect in Rickety complaints, that it does not follow as a consequence, that it is so also with the frac-

ture of a bone. Once more, on mentioning the foregoing ideas to Dr Manners, he suggested the propriety of extending this practice to the fracture of bones of Parturient women, for here no union will take place in the usual way, and why? Because all the bony matter is required for the formation of the bones of the fœtus, and of course none is left to produce a union in her own fractured bones.

This I think highly ingenious, and I would have done an injustice to the Author had I not mentioned it. Lastly, what would be the effect, if all Parturient females should live upon substances that have lime, as one of their ingredients, might not the fœtus be formed in a smaller space of time, and thus abolish the term of nine months.

REMARKS ON HEAT,

By Thomas D. Mitchell, M. D. *Fellow of the Academy of Natural Sciences of Philadelphia, Honorary member of the Columbian Chemical Society of Philadelphia, &c.*

CHEMISTS, in all ages have been studious in investigating the phenomena of heat. Its great and important agency in all the processes of art, necessarily rendered it a subject of material interest. It was not however, till the days of Dr. Black, that the subject assumed a philosophical attitude. This ingenious man, from the rude theories of the many who preceded him, collected the outlines of that system which is now so generally received.

I am not convinced, however, that the theory of heat, as it is called, has yet attained to perfection. And perhaps some of the apparent beauties of the system of Dr. Black, will some day, be viewed as scientific deformities. I was led to a more attentive examination of this subject from some remarks made by my friend Dr. Manners, in private conversation on the subject of latent heat. That gentleman stated his disbelief in the doctrine of latent

caloric in solids, and wished to have substituted the term specific heat. This much of the coversation, candor prompted me to state. After having reflected on the subject with some attention, I am induced to believe that there is no just reason for the existence of latent heat in solids. I do not beleive that Dr. Black originally meant any thing more by latent caloric than that quantity of the matter of heat which rendered bodies fluid. But the extent of his system necessarily involved him in some difficulties, and perhaps gave rise to a frequent misapplication of the term *latent caloric.* Even that part of the theory relative to the caloric of fluidity is involved in some obscurity. We are informed that a quantity of ice at 22° placed in a warm room, will rise to 32°, and then the heat will be stationary until the ice is melted, when it will rise in temperature. Now we are certain that in this case, caloric must have combined with the ice although the act of combination did not effect the thermometer, and we can only explain it, by supposing that the affinity between the ice and caloric was so great, as to render the absorption of the latter insensible, by its rapidity.

This, however, is a case in which we are compelled to give our assent, because experiment proves the fact, and reason cannot, as I believe, overturn it. If this concession in Science be necessary in one instance, it is not so in all. And I do not think there is any just cause for obscurity in the phenomena of caloric so far as regards solids. It is in my opinion very easy in a rational manner

to disprove the doctrine of latent heat in solid bodies.

Heat has been called by different specific names, as absolute, specific, sensible, latent, &c. And for each of these, there are perhaps several synonimes. Now I think, it will appear that the term *absolute heat*, is sufficient to express all that is desirable to be conveyed by the term *latent heat.* Understand me; I do not say that absolute heat is the same with latent heat, by no means; I wish to make it appear that the term latent heat has been applied to the heat of solids, when the term *absolute* should have been used. Thus for example it is common to say, a piece of iron contains a quantity of heat in a latent state, and that by being hammered this heat will be evolved and rendered sensible. Now I shall endeavour to make it appear that the heat was not latent, both according to Dr. Black's application of the term latent caloric and also from analogical reasoning.

In the first place then, I ask what do we mean by sensible heat? It is obviously that which is indicated by the thermometer. The thermometer will indicate the heat of one body as 180° of another as 20°, yet in the one case, we call the body hot, in the other cold. In the one case, common phraseology would endow the body with sensible heat, while in the other, caloric would be latent. Yet the heat of both bodies is distinctly indicated by the thermometer, the one as at 20°, the other 180°. Now although the body at 20° may be called cold, yet it is relatively hot, that is to say, it

possesses more caloric in a sensible state, than a body at 15°. When Dr. Black found that ice could be exposed to a great heat without affecting the thermometer, he called the heat which combined with the ice, latent, because it could not be detected. But this could not take place with other bodies as iron, and this would be increased in temperature by the smallest accession of heat. Yet the ice, as a solid is on a footing with the iron in this respect, that it contains, relatively speaking, a quantity of sensible heat. And it would appear that nearly all bodies while in a state of solidity are influenced alike by the phenomena of heat, but that latent caloric or as it is often properly called caloric of fluidity, is only concerned in the change of bodies from solidity to fluidity.

Absolute heat is a term used to denote the entire quantity of caloric in a body. Now it is common to say, on mixing two substances and thereby producing an extrication of heat, that the caloric is passing from a latent to a free state. I deny this however, otherwise there is no use in the term *absolute heat.* If the entire quantity of heat in one body be 20°, and that of another be 15, the heat of the two would of course be 35. But suppose that the mixture of these substances is attended with a great degree of condensation, it would then follow that the heat of the compound could not be 35, but something less in proportion to the condensation. Now this portion of heat which is thus given off is nothing more than a part of the absolute heat of both bodies, escaping in conse-

quence of condensation. A piece of iron at 20° contains its proper quantity of absolute heat, the measurement of which has never been accomplished. It is also evident that it contains all the sensible heat indicated by 20°. Suppose I subject this iron to the action of a hammer. The effect is obvious. I increase the attraction of aggregation between the particles of the iron, or I condense it.

By this condensation, the absolute heat cannot possibly occupy as large a space as before since the particles of the iron are forced into close contact. It must therefore escape, and in the act of its evolution, raises the thermometer and thus becomes what we call sensible heat. Therefore if the remarks now made be just, Dr Black is in an error when he says that iron loses its malleability by hammering in consquence of the loss of latent caloric. It loses its malleability in consequence of having been hammered, till it is no longer malleable. If there be a certain point of union, beyond which the particles of matter cannot be forced, and if by means of any power, we gain that point, the difficulty is at once removed, and we can easily perceive that too much hammering destroys the malleabillity of iron, only by forcing its particles into close contact, and of course that latent heat is not at all concerned in the process.

Thus far have I ventured to explain the subject under consideration, by the aid of Dr. Black's application of the term *latent heat*. I will now in

a brief manner attempt to establish my position by analogical reasoning.

I know the fallacy of reasoning from analogy. But nevertheless this mode of argument is sometimes conclusive.

I have said that by rendering a solid, as iron, more dense, we force out its absolute caloric, and that while we thus make a sensible increase of heat, we do not affect latent caloric in any way.

The specific gravity of bodies is always in proportion to their density. Hence the truth of the remark that if it were possible to compress a piece of cork into so small a space that its particles would be in as close contact as those of platina, we would thus increase its specific gravity in a remarkable manner. Now to what should we attribute the immense hydrostatic difference thus produced, but to a change in the aggregation of the particles of matter? we would not say that the latent gravity was evolved and rendered sensible by condensation. We would more justly conclude that by condensation, the specific gravity of one body was increased, and that by the same means the heat of another body was rendered more obvious.

It is laid down as a maxim, and perhaps there is none more true, that when bodies pass from a fluid to a solid state, heat is evolved. This plain proposition, is however, misunderstood by many. It is frequently asserted, that the heat which is sometimes evolved on the passage of bodies from fluidity to solidity is merely latent heat rendered

sensible. This I do not beleive. For if such bodies be condensed, the very act of condensation is sufficient to produce a decrease of the absolute heat, which in its escape, becomes sensible. It may be said that this rule is not applicable to all cases, since water is enlarged by freezing and not condensed. This however, is a rare exception, and it is a matter of doubt, whether water does not actually condense by freezing, even though its bulk be enlarged. For it is a fact generally admitted, that ice contains a large quantity of air, which not only explains its levity, but is likewise in my opinion, sufficient to satisfy any man of the error of those who suppose it to be enlarged in passing from a fluid state.

I do not deny that the latent heat of water may pass off when this fluid assumes the solid form; but I contend, that in all cases of this change, the heat which is perceptible, is the absolute heat, evolved by condensation, and not as some suppose, the latent heat, rendered sensible.

ON THE OXYACETITE OF IRON

As a test or reagent for the discovery of Arsenic.

By James Curbush. *Professor of Chemistry, &c. in St. John's College.*

THE importance of obtaining correct deductions, in experimental inquiries, is acknowledged to be of considerable utility in the practical researches of the chemist. Especially in the analyses of bodies, whether solid or fluid, our attention is directed to the employment of a number of *reagents*, which are to determine, beyond the possibility of doubt, the presence, or absence, of certain bodies.

Reagents are, as Mr. Accum so justly observes, the "compass by which the Chemist steers." When chemistry shall have arrived to the summit of perfection, it will be then, and not till then, that we shall draw conclusions, which the power of genius can not overthrow; and as we progress in this knowledge, the more we would be able to deter-

mine the correctness of any deduction founded on the immutable laws of nature. As to the reagents employed for the detection of *arsenic* it is well known that a number have been recommended. I have been led to make some experiments with the oxyacetite of iron, as a test for arsenic, from the circumstance that iron and arsenic, in peculiar states of combination, form an *insoluble compound of a brilliant orange tint.*

We shall first, however, offer some remarks on the preparation, and then describe its application.

When iron filings is degested in acetous acid (distilled vinegar) a portion is dissolved untill the acid is destroyed, a compound being formed of acetous acid and iron. During this operation, the iron is oxydized, to the minimum only, at the expense of the water in the acetous acid, which is then taken up by the acid, forming the acetite of iron: at the same time a portion of hydrogen gas is evolved in consequence of the decomposition of the water. Or, if acetite of lead *(sugar of lead)* and sulphate of iron *(coperas of the shops)* be mixed together in solution, a double decomposition will ensue, sulphate of lead will be precipitated, and acetite of iron be held in solution. This solution of iron is well known in the arts, by the name of *iron liquor.* It is also prepared by dissolving iron in the pyroligneous acid, which is obtained by the distillation of wood.

But when the per oxid of iron, or iron oxydized to the maximum, is combined with the acetous acid, another compound is formed, called *oxyace-*

tite of iron, which I propose as a test for arsenic. This preparation I made in the following manner:

Equal parts of the red or oxysulphate of iron and acetite of lead, were mixed together in a sufficient quantity of water, and the sulphate of lead, which was precipitated, was separated by the filter. The oxyacetite of iron, which passed the filter, was of a beautiful reddish-brown colour.

This solution, added to another of arsenic, gave a precipitate of a beautiful orange test, which, on examination, proved to be a compound of arsenic and iron; for on exposing it to heat, the arsenic was volatalized, whilst the oxid of iron remained behind.

From an experiment made to ascertain the *per cent.* of each metal, in the precipitate, I found that my experiment indicated the following properties:

Arsenic	.55	Or arsenic	40
Iron	.45	iron	35
		oxygen	25
	1 00		1 00

It may not be improper to state, that the oxysulphate of iron was prepared by digesting in a dish, eight parts of the green sulphate and two parts of nitric acid; a sufficient heat being applied to decompose the acid. In this case, a part of the oxygen of the nitric acid united with the iron of the sulphate, forming the per oxyd, whilst the other part was disengaged in union with the azote in the form of nitric oxyd gas or nitrous gas.

The per oxid in union with the sulphuric acid, formed the oxysulphate of iron.

If arsenic in the state of acid, whether the arseneious, or arsenic, be combined with potash in the form of arsenite, or arseniate of potash, and added to the oxyacetate of iron, a double decomposition would ensue.

The difference in the colour of the precipitates in the combinations of the arsenic and the iron, depends on the degree of oxydizement in the solution of iron. Hence it is, that with the common acetite of iron, in which the protoxyde of this metal is dissolved, the colour of the precipitate produced with the arsenious acid, or the common white arsenic of commence, is of a *greenish-yellow*, the precipitate from the oxyacetite being of a bright orange. If either of the acetites contain the least portion of the sulphate of iron, it prevents the precipitation of the arsenite.

Hence also, if arsenic acid (in which the dose of oxygen is at the maximum) be present in a fluid, thecolour produced with the oxyacetite of iron will be of a blueish-white.

Therefore, in common will all the other salts of iron, the arseniate of iron will combine with an additional dose of oxygen, constituting the oxy-arseniate. Richard Chenenix Esq. found, that the oxy-arseniate of iron contains

Acid	42.4
Oxyde	37.2
Water	29.4
	1 00

On the same principle as the oxyacetite of iron is used as a test of the discovery of arsenic, Scheele I think recommended some of the solution of copper. If acetite of copper (*distilled verdigrice* of the shops) be added to an arsenical solution, the arsenic will be precipitates in union with the copper, forming the *Scheele's green*. This precipitation of arsenic by solutions of copper, has rendered the latter a test for arsenic. Other reagents for discovering this metal have been recommended.

THOUGHTS

ON THE

PRINCIPLE OF EXCITABILITY.

By George Ferdinand Lehman. *Vice President of the Columbian Chemical Society, Honorary member of the Medical Society of Philadelphia.*

THE important discoveries, of late years in the science of medicine, and chemistry, have changed the complexion of nearly all that was before known. In ancient days, when science was just evolving from darkness, nothing like perfection could be expected, from the sons of philosophy. Many of them however, entered the labyrinth of nature, and gave to the world her secrets. They had no marks to guide them, and were carried along by impulse, through regions of truth, and fiction. These uncertain wanderings brought within their grasp facts, though in a confused state, which served to enlighten the path of modern philosophers. The sweets of the ancients, have been culled and improved. Among the various phenomena which appeared to them, life was the prin-

cipal. The materiality of it was contended for by the prophets. Oxygen gas with which we are now so well acquainted, was the *pneuma*, which Aristotle thought existed in the air, and combined with the blood. Chrysippus and Bacon were materialists.*

The importance of excitability must be obvious to the most superficial observer. It is the medium through which stimuli act, to excite the mind, and body. Where would have been the sublime works of a Milton, Pope, Cowper, and Goldsmith, had they been devoid of this capacity? Where the useful discoveries of Lavoisier, Priestley, Franklin and Rittenhouse? They would all be covered in mystery and darkness. The lasting monuments of science, erected by the industry and perseverance of the children of genius, would be wanting. The face of nature would consist of nothing more than a mass, of inanimate matter, deprived of it. Wherever life, or motion exists, there must be a capacity of being acted on by stimuli. The most enlightened philosophers of the present age admit that life is a forced state. Some writers on animation, affirm that we cannot admit of a vivifying principle; if this theory be correct. That indeed would be the case were we born with perfect life, when in fact it is only a capacity of life. It is certainly at variance, with Haller's Vis Vitæ, and Cullen's Vis Medicatrix Naturæ. The best mode of studying, and understanding the laws of Nature, is by driving her out of her uni-

* Johnson's Animal Chemistry.

form track by experiment. This as it applies to the human body, in the present inquiry, is impracticable. We must reason principally from facts, experiments performed on the brute creation, and analogy. I have always been of opinion, that oxygen acted more than a negative part, in the system; but it would be too tedious at this time, to mention the facts on which this belief is founded.* That oxygen is the substance from which excitability is generated, appears probable from the following circumstances.†

1. Respiration as a universal function throughout animated nature, first attracts our regard. We are convinced, from the effect of placing animals in vacuo, or of tying the trachea, that it and life are intimately connected. In what consists the necessity, of this connexion? Does the air continue existence, by stimulating the system? or does it only serve for the production of animal heat? In answer to the preceeding question, it is sufficient to observe, that incetants of the most powerful kind applied to every part of the body, in a torpid state, as Adam was before he received into his nostrils, the breath of life, will not produce vitality. If it were only requisite to the production of heat, animals deprived of respiration, and exposed

* Although some of the greatest English chemists, (among whom may be ranked Murray, and Thompson) stand in defence of the negative theory: their reasonings, and deductions, are not sufficiently weighty, to prostrate that promulgated by the Chemists of France.

† Girtanner is the firmest supporter of this opinion.

to a temperature equal to the blood should live considerably longer than those in a cold situation. Experiments convince us that this is not the case. They die nearly at the same time; but we sometimes observe heat fluctuating in the body hours, and even days after death, which shews that the system is not indebted altogether for its caloric to this function, and that it is not connected with life, in this way.

Is the oxygen of the atmosphere, absorbed by the blood, and conveyed to every part of the body impregnating it with vitality? Venious blood, or that on which the air has not acted, will, in a very short time destroy life. Nitrogen, and oxygen with a small portion of carbonic acid gas, are the substances composing the atmosphere: consequently it must be owing, to one or all of these gases impregnating the blood. Neither carbonic acid gas, nor nitrogen will support animation, nor will the combination of any other gas with nitrogen (except oxygen.) As but a small portion of azotic gas, is absorbed, and if the experiments of Pepys, and Allen be correct, none at all* it cannot therefore be considered the active ingredient. Is it oxygen? no other explanation, can reasonably be offered.

2. If we place animals in oxygen gas, they live, and seem to do tolerably well; but after a certain time their vital energy in every respect is increased: this is explained exactly in the same manner,

Pepys and Allen on Respiration.

as blowing a fire with a pair of bellows; here fuel is added to flame.

Dr. Hare mentions in one of the volumes of the Philosophical Transactions of London; that mice, birds, &c. live as long again, in a vessel where he had crowded in double the quantity of air by a condensing engine, than they did when confined in air of the common density. This clearly evinces to us, that something must have been derived from the atmosphere necessary to sustain life.—Was it not oxygen?

The inspiration of nitrous oxyde gas, affords an argument in defence of this opinion, it excites not only the mind and muscles: but imparts vigour, and force to every part of the human frame. The celebrated Davy, after he had inspired it exclaimed, "nothing exists but thoughts, the universe is composed of impressions, ideas, pleasures and pains." Others rave about like madmen. Is this increase of vitality, owing in part to the absorbtion of oxygen? This is rendered probable from the succeeding facts.

Davy inspired 102 cubic inches of this gas, mixed with one fiftieth of common air. Forty inches disappeared, and the remainder sixty-two inches by analysis consisted of

Carbonic Acid	3.2
Nitrous Oxyde	29.0
Oxygen	4.1
Nitrogen	25.7

Thus you perceive, that nearly all the oxygen was absorbed. This however, is not altogether

satisfactory; if it were the inspiration of oxygen gas alone, should have these effects. Owing to the proportional combination, of the gasses forming nitrous oxyde, it has a peculiar power; it elevates the nerves, to their highest capacity; but it is that kind of elevation, which is not succeeded by debility. The blood of animals killed by breathing this gas, is a purple red, spots of a similar colour are to be seen in the lungs; and the muscles to all appearance possess great irritability.*

It is the known effect, of the oxygenous principle of the atmosphere, when duly inhaled from a current breeze, not only to saturate the blood with floridity; but to exhilerate the spirits in a high degree, invigorate, and redden the muscular fibre: bestow energy on every nerve, and call forth at once mental, and corporeal strength."

Hence the necessity of a pure air is obvious. Why do we so often observe people fainting, in in crowded rooms? and why does the introduction of fresh air, immediately restore them? Can these questions be answered, in any manner consistent with reason, unless we resort to the agency of oxygen. Is not the syncope the effect of the deficiency of vital gas? The gaping and yawning which takes place in large assemblies, I have no doubt is owing to the same cause. The uneasiness in the heart and faintness, which has been experienced by those ascending high mountains, is to be explained in a similar way.

* Davy. Trotter on Nervous Temperament.

3. The wavering state of excitability, is in support of it. This is obvious sometimes in health, but most frequently in the diseased state of the system. In an inflammatory fever, the pulse is generally full, and quick: the eyes and face suffused with blood, the respiration is frequent, and often laborious; the whole body is excited. Is there any necessity, for this quick inspiration? Certainly, the different stimuli acting on the body in the disease are so great, that the excitability would soon be expended, and death would be the consequence if there were not a preternatural supply of vital air. Whence does the system obtain this addition of stimulability? is it not from the greater quantity of oxygen conveyed into the lungs? on the contrary in Typhus fever, where the pulse is weak, and feeble, countenance pale, and respiration performed slow, and languid the patient is weak, and almost lifeless.

Asthmatic patients long very much for pure air; because the air vesicles of their lungs are filled with serum, and the air cannot come sufficiently in contact with the blood. These people are generally cold, and in a weak state, the pulse being very slow.* Does not this depend on a less quantity of oxygen in the blood? I have merely specified these cases, to render the position more obvious; it applies generally to all diseases of high, or low morbid action.

* Bree on Asthma.

6. "Birds whose pulmonary organs, are continued into the abdomen by several membranous sacs, and whose hollow bones communicate with the lungs; consume a great quantity of oxygen, either on account of the extent of their pneumatic receptacle, or because their respiration is frequent, and often sudden; thus the habitual temperature of their bodies is ten degrees above men, and the Mammifery or those having teats." They are extremely active, and energetic and seem to possess a superabundance of life. Those birds which possess most muscular power, and run about as soon as they escape from the shell, are generally supplied with a larger receptacle for air in the shell than these dependent for their subsistence after being hatched; now as this air is allowed to be oxygen does it not generate excitability to be acted on by stimuli. "In reptiles, on the contrary, the vesicular lungs of which, receive only a small quantity of blood, and offers but a small surface respiration takes place at more distant intervals, consequently they have a temperature never above seven, or eight degrees.* And it is scarcely necessary to mention, that their motions are for the most part slow, and languid. Does not this point out an intimate connexion between life and oxygen?

7. By injecting this gas into the blood-vessels in a quantity sufficiently large to produce death, the heart, and muscles possess irritability in a de-

* Richerand's Physiology.

gree considerably greater than where life is extinguished from any other cause. Does not this render it probable, that the vital principle is oxyen? If the other gasses are employed, death will be the consequence, and the irritability of the muscles has decreased, or nearly wasted. Is not this another stone added to the foundation? Why should the muscles have such a susceptibility to the action of stimuli, after the application of oxygen only?

8. Black blood, or that destitute of oxygen is fatal to animal life; this is clearly proved by an experiment of Bichat. "I opened," says this industrious author, the "carotid, and jugular of an animal; and received in a syringe heated to the temperature of the body, the fluid poured out by the latter, which I injected into the brain of the former which had been tied on the side next the heart to avoid hemorrhage, the animal was almost immediately agitated, he appeared to be undergoing similar sufferings to those produced by asphixia, of which he very soon shewed all the symptoms: animal life was entirely suspended, the heart still continued to beat, and the circulation to go on for half an hour at the end of which time death terminated organic life also." "The black blood," continues he, is no doubt fatal to the brain by striking it with atony, by its contact," or in other words, abstracting its vital power, rendering it incapable of motion.

We can inject the arterial blood of one animal, into the carotids of another, and it will not cause death; this shews that the consequence of inject-

ing black blood, into arteries does not depend on any change which could take place in this fluid passing from one body to another; if this were the case death should occur in both instances.

9. By impeding the progress of arterial blood to a limb, or any part of the body, the excitability is destroyed, and partial death occurs. It is also evident in aneurisms. When an artery is taken up, the death of the extremity sometimes succeeds and in a very rapid manner. This cannot be the consequence of the loss of motion by the deficiency of blood, or any other fluid equally stimulating should preserve life, nor can it be owing to the want of nutritious substance, because this is an extremely gradual process, and its absence could not be so suddenly perceived.* It is the want of excitability; and I have no doubt but one third of the blood, which is naturally destined for the support of a limb, or in fact the whole body, would be sufficient if highly oxygenized.

In youth, the arteries are larger than in advanced life, and the blood which is peculiar to them is great in quantity, hence young people are generally vigorous, and active; but in old age the arterial system dwindles, while there is an enlargement of the venous, need I add that debility, languor, and paleness accompany it.

10. The oxydes of iron, and steel are particularly useful in diseases of great weakness. May not this be owing to their capacity, of combining

* Bichat's Physeological Researches.

with a much larger quantity of oxygen, than most of the other metals; consequently affording a greater proportion to the system. The effects of these medicines, are, to increase the pulse, and produce a lively florid appearance in the face. Professor Coxe observes, that it is highly probable, iron would have no action on the body when taken into the stomach unless oxydized.*

11. The pulses of the generality of people are slower in winter than they are in the warm season. This perhaps may be accounted for, without resorting to the sedative action of cold. It is known that the chief source of vital air, is from the vegetable kingdom. In the cold season of course this great spring is almost entirely cut off. The atmosphere probably contains a less quantity of oxygen gas. I am not sure whether any judicious experiments have been made, to ascertain this point with accuracy: however a very small deficiency of oxygen, might have the effect, and if no great change could be perceived by our uncertain experiments, the assertion would not be unreasonable. It is not as inconsistent, as to suppose that no impurities exist in the air, during the prevalence of a malignant, or epidemic distemper because they cannot be discovered by the eudiometer.

12. The inhabitants of the cold countries, of Lapland, and Iceland are by no means as lively, as those of warm climates. Blumenbach asserts, that

* Coxe's Dispensatory.

the heart of a Greenlander, in consequence of the coldness of the climate, pulsates only 30 or 40 strokes in a minute.*

Does not this depend on a less quantity of oxygen in the air? These observations it must be confessed are only conjectural; but we should attend even to conjectures, when countenanced by appearances, and analogy. Like the *Ignis Fatuus*, which frequently leads us into bogs, and quagmires, yet occasionally, conducts us through the mists of night with safety; they will serve, if true, for the elucidation of many phenomena, in the healthy, and morbid states of the body.

13. It is observed by Dr. Rush, that we make longer, and fuller inspirations in the sleeping, than waking state. This, by admitting into the lungs, a greater proportion of vital gas favours the accumulation of excitability; hence one of the reasons why diseases so frequently attack us at night,

14. In animals in utero, there is but a small quantity of excitability; but it is sufficient as in this situation, there are no powerful stimuli or exertions to expend it. That they possess less vital power, is evinced by suffering brutes to bring forth their young in water, and in common air; by placing the last directly in water, if it has ever breathed, you will perceive its struggles and emotions are much greater than the former. How can this fact be explained unless we admit oxygen, to

* Blumenbach's Physiology, Caldwells's Translation. p. 79.

be the source of stimulability? In the latter the quantity is increased, it therefore requires more violent actions to waste it. To shew with more precision the correctness of this assertion respecting the expenditure of excitability, I obtained two cats, and having immersed one in water, for the space of one or two minutes, I poured over it some hot water; the effect was very inconsiderable, it scarcely moved. By merely dropping it on the other, great agitation was the consequence. The irritability of the cat half drowned, was unquestionably diminished, and this diminution was the effect of the stoppage of respiration, preventing the absorbtion of oxygen; while at the same time, there was a waste of vital power, by the actions of the cat in the water.

15. The weariness, and debility produced by excessive exercise, or labour can be explained only on a supposition, of the wate of this principle. The circulation, from the pressure of muscles, agitation of the body, and other circumstances, is increased, to a very considerable degree. Upon this account a greater proportion of blood is sent to the lungs than usual. It is again hurried out of the thorax, only in part oxygenated, and so carried throughout the system. If this action continues a sense of suffocation occurs, and the subject soon falls, by the expenditure of excitability, or by the inability of the lungs, to supply the blood with vital gas to keep up animation. To render these observations almost indisputable; it is only necessary to take two animals cœteris paribus, and

by irritating one of them, keep it in continual motion for some minutes; and let the other remain in a state of quiescence. Then place them in air tight vessels, and the latter will be seen to live longest. Many of the effects of the stimulating passions, might be solved in the same manner.

16. Those people who possess broad chests, are for the most part strong and healthy. They generally are of a rosy appearance, and frequently possess great vivacity. On the other hand, view the small, and slender made. They are pale, and not very excitable.

Is it not probable, that this may be the consequence of their inacpacity of inhaling much air? and doos the liveliness florid countenance, &c. accompanying the broad chested man, depend on the large inspiration of air? In animals in which the pulmonary system is most complete, and spacious, life is greatest, or in other words, the capacity of life is in the highest degree.

17. The phenomena of plants, though it may not be a fact directly in point, perhaps will reflect some light on the subject. We know they are possessed of irritability, and other powers belonging to animals. Now if oxygen, is the principle of animation in one, it must be so in the other. This idea is not invalidated, from the circumstance of vegetables flourishing and doing well in *fixed air.* The life of plants is on a much lower scale than animals, hence the deprivation of oxygen to them by the air, is not attended with immediate bad effects. Reptiles are

nearly on a par with them in this respect. Plants have the power of abstracting vital gas from the water, carbonic acid, and others of their nutritious substances; which maintains their irritability. Vegetables placed in oxygen gas as pure as it can be obtained will not long survive in this situation. Horticulturists are so well acquainted with this fact, that they not unseldom place a glass vessel over young plants to prevent its contact. A moderate quantity of vital air, is favorable to the growth of plants.

18. Water in all probability is decomposed as it comes in contact with the animal fibre; and this is another way by which nature, affords the principle of life to animals. The rotifer which is a curious insect, after being dried, by moistening it with water, returns to life. This certainly must be owing to the oxygen of the water affording excitability, consequently life.

Fishes decompose water, with great facility even without the influence of their gills, as has been shewn by the experiments of Provencal and Humboldt. The carbonic acid they reject, does uot equal more than $\frac{4}{5}$ of the oxygen absorbed: hence their extreme vigour, and activity.*

19. It is well known that the white organs, and other parts of the body in a healthy state, have but a small degree of sensibility; but when inflammation occurs, and blood is propelled to them with velocity, their vitality is amazingly increased.

* Memoires de la Societe D'Arceuil.

Does not this arise from more oxygen being sent to the part? Let us inquire, how far this idea is corroborated by reason. In the natural state of the white organs, a light fluid circulates through them, and they are nearly destitute of excitability; inflamed their functions are materially altered. What before could not be affected by the most powerful stimuli, are now acutely, sensible to the slightest impressions. Red blood traverses the canals, which were before occupied alone by serum. The vital power is in proportion to the red blood present.

20. Hybernating animals, possess vitality in a very low degree during the winter season, as frequent, experiments demonstrate. If powerful irritants are applied to them, they either have no effect at all; or owing to the disproportion of stimulus, and excitability, the death of the animal is the consequence; but if left alone and undisturbed, when spring approaches, bearing on his wings the gentle gales of Heaven, and the vegetables begin again to impregnate the atmosphere with vital gas? These animals feel the change, inspire new life, and are excited into motion, and activity.

It has been asked if oxygen is a stimulus. It will kill if used in excess, but this abstracts nothing from the solidity of the idea, of its being the vital principle; because it is not supposed that oxygen gas, simply produces the vital power, but that a combination takes place in the animal fibre. The most natural stimuli under similar circumstances, will have an analagous effect. Thus the

mind of man, becomes shattered, and deranged by the preternatural predisposition of blood to the brain, a certain degree of which, is necessary to existence. Thus the burning wick, is extinguished by the application of too much oil, and the ship upset by the raging winds. Every philosopher knows, a definite quantity of oxygen gas will increase the stimulability of the body.

I shall now offer a few thoughts, on the origin of excitability.

It was long believed, before and even after we were taught to look externally, for the cause of life, that the brain was the grand reservoir from which the body derived its existence. This opinion has been sanctioned by men, who shine conspicuous in the temple of Science. Darwin observes, "As the sensorial power, or spirit of animation, is perpetually exhausted, by the expenditure of it in fibrous contractions; and is perpetually renewed, by the secretion, or production of it in the brain, and spinal marrow. The quantity of animal strength must be in a perpetual state of fluctuation on this account."

If it were necessary, we might bring forward evidence of a later date. Motion can be excited in the heart, and other muscles, by the application of stimuli, a considerable time after death. Bichat, who was permitted to experiment on the bodies, of those unfortunate beings who fell beneath the guillotine in the year 1798 in France, produced motion in the muscles, by irritants long after the head had been separated from the trunk. There are

animals destitute, of brain altogether; and who has ever discovered the sensorium of plants, yet no one will deny that they possess irritability. Turtles, and frogs, after their heads have been cut off, live hours, days, and even months. The different pieces of polypi will live after being cut asunder; and the heart in the growth of the fœtus, is first formed.

These facts prove that the brain, cannot be the fountain of excitability. Whence then does it originate? Every part of the animal frame, is endowed with excitability, though in some it is scarcely perceptible. Those parts which have most of it, are supplied with the greatest quantity of arterial blood, and it is distributed through every minutiæ of the body, by the small capillary vessels.* Does the oxygen gas unite with the animal fibre, in the same state that it entered into the circulation or does it undergo a decomposition? Can it combine with the nitrogen of the fibre, in such a proportion, as to produce something allied to nitrous oxyde gas. This seems probable from the phenomena attending the inspiration of this *gas*. Here let us cease the strain of conjecture, "shadows, clouds, and darkness rest upon it."

* Dr. Samuel Jackson, to whose ingenious dissertation I am indebted for several facts in this essay.

ANALYSIS

OF A

MINERAL SPRING,

At the Willow Grove, Montgomery, County, Pennsylvania. By JOHN MANNERS M. D. *and* THOMAS D. MITCHELL M. D.

THIS spring is distant from Philadelphia about 14 miles, and in a country, where the soil is extremely stony. Nothing, however, very remarkable, presents itself to view in the vicinity of the Spring.

In order to analyse the water satisfactorily, it was examined at its sources. It exhibits no very remarkable appearances, except that in color it is rather of a blueish cast. It is covered frequently in the course of a day, with a thick scum, indicative of the existence of sulphuretted Hydrogen.

In the analysis, the following tests were employed and their effects noted as follows, viz.

1. Litmus Paper....No change.
2. Turmeric do. do.
3. Tinct Galls....A dark purplish red color.

4. Prussiate of Potash....A lightblue which became much darker by the addition of sulphuric acid.

5. Sulphuric acid....No change.

6. Muriatic acid. do.

7. Nitrate of Silver....A white precipitate with a greenish cast.

8. Sulph. of silver....A white precipitate which became purple in a short time.

8. Acet Plumbi....A slight turbidness.

10. Mur. Barytes....No change.

11. Nit. Mercury do.

12. Acet. silver....A slight turbidness.

13. Tinct Soap do. do.

14. Ammonia....No change.

15. Mur, Hyd....No change.

16. Oxalic acid do.

On shaking the water it emitted an Hepatic smell.

Some of the water was brought to the city, and three ounces of it were boiled down to 2 ounces; then it was tested for alkali, but none could be detected. Pruss. potash added, gave a blue precipitate much increased by sulphuric acid. This was suffered to stand 24 hours, at the expiration of which a copious precipitate had fallen down, which proved to be prussian blue, weighing 6 grs. From the above analysis, the following inferences are necessarily drawn.

1. The non-existence of carbonic, or any other acid in an uncombined state.

2. The presence of iron is fairly established.

3. The water contains sulphuretted Hydrogen, which is evinced both from the effects of agitation, and the results of the tests employed.

4. No copper exists in this water, as is proved by the use of ammonia.

5. It is not found to contain the smallest quanty of lime.

AN ENQUIRY

Whether MR. BERTHOLLET *was warranted, from certain experiments, in framing the Law of Chemical affinity, "that it is directly proportional to the quantity of matter."* BY FRANKLIN BACHE, *Vice President of the Columbian Chemical Society.*

We shall place the subject under three heads: in the first place, we shall attempt to prove that his experiments did not warrant him in the conclusion which he has drawn. 2d. We shall consider what is the proper conclusion, and 3d. how it happened that Mr. Berthollet was so egregiously mistaken in the tendency of his own experiments.

We shall premise our observations with an extract from Thomson's Chemistry; this distinguished chemist speaking on this subject, has the following words. "And it has lately been demonstrated by Berthollet to hold in every case: thus a given portion of water is retained more obstinately by a large quantity of sulphuric acid than by a small quantity. Oxygen is more easily abstracted from those ox-

ides, which are oxidized to a maximum, than from those which are oxidized to a minimum; that is to say, that a large mass of metal retains a given quantity of oxygen, more violently than a small mass."

The above are instances, which Mr. Thomson gives as proofs of the law, as Berthollet stated it. Let us examine whether they are really proofs. Let us trace him through his reasoning. He says "a given portion of water is retained more obstinately by a large quantity of sulphuric acid than by a small quantity;" therefore, he concludes that chemical affinity is directly proportional to the quantity of matter—but to prove the fallacy of this conclusion, let us view his proposition in another light equally true; it is this; a given quantity of sulphuric acid is retained more obstinately by a small quantity of water, than by a large quantity; therefore, we conclude that chemical affinity is inversely proportional to the quantity of matter; but this is a conclusion diametrically opposite to that which Mr. Berthollet draws; how is this to be explained, both conclusions cannot be true? we answer, by proving that Mr. Berthollet's proposition is not universally true, but under certain circumstances only, which circumstances are that the two different portions of the body, compared with any other, must be both greater than that quantity which would constitute a minimum of attraction, between the body of which we take the two portions and the third, with which it is compared, or if not both greater, but one less and that not as much below the minimum as the other is above: we will explain ourselves by an example, before which we must fix

for our convenience an arbitrary point, at which this *minimum* of attraction takes place; we say arbitrary, because, nothing but experiment can decide it, and that we are not prepared to make; we will state this point for our convenience, at equal *quantities*, thinking it more probable that experiment would establish it there; now for example, let us once more take Mr. Thomson's own words, "a given portion of water is retained more obstinately by a large quantity of sulphuric acid, than by a small quantity." The question is, is this true? we will prove it so only in particular cases; and the particular case in which it is not true, is the following; let us take two pounds of water, in separate vessels, and two portions of sulphuric acid, namely, 20 grains and 4 drams: to one vessel we will add the 20 grains of sulphuric acid, to the other, the 4 drams. Now the attraction between the water and the 20 grains is greater, than between an equal quantity of water and 4 drams; that is to say, a given portion of water is retained more obstinately by a small quantity of sulphuric acid, than by a large quantity. This is the conclusion from this particular instance, which, as we premised, is diametrically opposite to that drawn by Berthollet. It may be asked, what right we have to conclude as we have done? we answer, to avoid the error into which Berthollet has fallen, that of considering water *inert*; for the sulphuric acid is as much retained by the water, as the water by the sulphuric acid; their action is reciprocal, their union chemical.

We have mentioned the example in which the proposition is not true, and we state it in words, not to be true, in all those cases in which the compared portions are both under the *minimum* of attraction, or if not under, but one above, and less above than the other is below. Let us illustrate by example; thus, having premised that the attraction between equal quantities of bodies combining in indefinite proportions is at a minimum:

Therefore the affinity between a pound of water and a pound of sulphuric acid, may be expressed by . . 0

Then dividing the grades betweeen the minimum and maximum into 8.

Then the attraction of 14oz. of sulphuric acid and a pound of water or 14 oz. of water, and 1 lb. of sulphuric acid would be 1

That of 12 oz. of sulphuric acid and 1 lb. of water or 12 oz. of water 1 lb. of sulphuric acid . 2

Thus also the affinity of 20 oz and 12oz. of sulphuric acid, would be the same for a lb of water. So that we may safely conclude that the real law is as follows, namely, that chemical affinity encreases between any two substances, in proportion as the ratio in which they combine, differs from that ratio of combination at which the attraction is at a minimum.

There can be no doubt that such a minimum exists; for we know that a few drops of water, and

a pound of sulphuric acid, as well as a few drops of sulphuric acid and a pound of water, both adhere more strongly than equal quantities; hence there must be an intermediate point at which the attraction is at a minimum; again, we do not pretend to say, in what ratio the affinity encreases, whether it be in the duplicate or triplicate ratio of the degree of variance from that proportion constituting a minimum.

This view of the subject leads us into the following paradox; that any repulsion between two bodies less than infinity, may be converted into any attraction, less than infinity, by making the ratio in which they combine infinitely to vary from that ratio of combination at which the minimum of affinity would take place.

It may not a little contribute in enforcing the position here taken to point out the probable way, in which this great Philosopher fell into this great error—we would explain it thus; it is with chemical combinations, as is the case with other natural phenomena, that in proportion as an operation is common, or its ingredients familiar, to us we do not regard it with as much pleasure, or in a philosophic point of view; thus it is that chemists too frequently forget the chemical agency of water; this we have no doubt was the case with Berthollet; he found that the less the quantity of water in sulphuric acid, the greater the difficulty of separaing it; that the difficulty of separation increased, in proportion as pure sulphuric acid was added, because he plainly saw it amounted to the same thing

as the water decreases. Well then, said he, as the attraction in this case, is increased by adding pure sulphuric acid, as sulphuric acid is matter, therefore it is increased by adding matter; and in proportion to the quantity added too; it was by such a train of reasoning, that Berthollet was induced to frame this law, and no doubt, he supposed it of as much importance to chemistry, as an analogous one discovered by Newton, is to the explanation of the motion of the heavenly bodies; thus it is perceived that Berthollet never thought of the water; never thought, that, while he was proving one law by adding sulphuric acid that he might, with equal propriety, have demonstrated its contrary by increasing the water.

ON MURIATIC

AND

OXY-MURIATIC ACIDS, COMBUSTION,

&c. &c.

By Thomms D. Mitchell, M. D. *Fellow of the Academy of Natural Sciences of Philadelphia, Honorary Member of the Columbian Chemical Society of Philadelphia. &c.*

ON MURIATIC, AND OXY-MURIATIC ACIDS.

The nature of these acids has been the subject of frequent investigation.

I will not attempt to enumerate all the theories which have prevailed on this subject. Many years ago, when phlogistic chemistry was more prevalent, Mr. Scheele supposed that muriatic acid was a compound of oxy-muriatic acid and phlogiston, and that the former was procured by depriving muriatic acid of the latter. Hence the name *de phlogisticated muriatic acid.* If we examine the grounds on which these conclusions were founded, they appear to be, chiefly, a desire to establish the exis-

tence of phlogiston, as a principle of inflammability. This will be evident in the following lines. "When muriatic acid is dephlogisticated with as much force as possible, it attracts phlogiston, with great avidity, dissolving metals by its affinity for their phlogiston, and thus uniting the inflammable principle to itself resumes the ordinary form of muriatic acid." Again, "The phenomena attending the inflammation of metals in oxy-muriatic acid, proceed from the great affinity of this acid for phlogiston. Marine acid which contains phlogiston as one of its component parts, must necessarily have little or no effect on the metals which retain their principle of inflammability, very obstinately."*

Now from all this it is abundantly evident, that Scheele's theory of the nature of muriatic acid arose from his firm belief in the existence of phlogiston as the inflammable principle. So tenacious are some men of a favorite theory, that in order to support it, they are willing to make every thing else subservient to that end.

Mr. Davy has lately investigated this subject and his conclusions are essentially the same with those of Scheele. He has appeared the champion of the Phlogistic system and has endeavoured to discover hydrogen in almost every thing.

But how does he support his opinions or rather the opinions he advocates? I answer; by storming with all the fury of his Galvanic Battery, the

* See Dobson's Encyclopedia.

fair edifice of Lavoisier: an edifice, which had its author lived to complete it, would have defied the attacks of its enemies forever.

If the theory as first given by Scheele and now supported by Davy, be true, the former deserves the credit of having maintained it more ably than the latter. He has attempted to support it in a manner, which would, at first view, appear quite satisfactory. But as his premises are false, his conclusions must be incorrect; for he has taken as granted, that which has not been proved, viz. the existence of phlogiston.

Mr. Davy certainly discovers much assurance when he boldly sets up his judgement in opposition to that of thousands, and that too on obvious points. He tells us "that chemists are mistaken in supposing that muriatic acid gas contains water." Now for this assertion, he has no proof either in philosophy or fact.

"It has been said," says Davy "by chemists, that when oxy-muriatic acid and ammonia act on each other, water is formed," but from experiment, I am certain this is not the case," and what is the experiment by which the ignorance of other chemists is to be so ably displayed? it is this; "when about 15 or 16 parts of oxy-muriatic acid gas are mixed with from 40 to 50 of ammoniacal gas, there is a condensation of the whole of the acid and alkaline gasses, and from 5 to 6 parts of nitrogen are produced; and the result is dry muriate of ammonia."

But, I ask, how does this furtile experiment prove that water is not formed by the hydrogen of the decomposed ammonia uniting to the oxy-muriatic acid? what becomes of the hydrogen if it does not combine with the oxygen, and thus in the form of water exist in the mur. ammonia? will we be told, as we have been heretofore, that the hydrogen united to the oxy-muriatic acid to form muriatic acid? the position is absurd, and is at best merely a conjecture, as such an union has not been demonstrated. It is more plausible, that the hydrogen and oxygen gasses should have combined to form water. According to Mr. Davy's experiment, hydrogen has disappeared, and it is sufficient for us to believe that it has entered into combination with the oxygen of theoxy-muriatic acid and that it exists in the state of water in the salt produced. This experiment, therefore, is far from disproving the compound nature of oxy-muriatic acid, and it is in my opinion, the best proof that has been adduced, since all else is wild conjecture. Mr. Davy, indeed asserts "that no substance known to contain oxygen can be procured from oxy-muriatic acid."

But in every case where metals are acted upon by oxy-muriatic acid, muriates are formed. Now I ask, whence comes the oxygen in an oxyde of gold, combined with mur. acid, after combustion in oxy-muriatic acid, if not from the oxygen of the latter? can it proceed from the muriatic acid? certainly not, since this acid is not capable of acting on gold.

That oxy-muriatic acid should be a simple substance appears to me as ridiculous as it is untrue. How does Mr. Davy evade the common opinion of the decomposition of this acid by light and other means? why to be sure, he tells us that something must always be present to furnish hydrogen and thus to form muriatic acid. But this rests altogether on supposition and has not been proved by direct experiment.

After all that has been said of theory, and its fallacy, it may appear strange that I should attempt to offer further conjectures on this subject. But there is a period in the philosophical investigation of every subject, at which experiment fails to be useful. And it is sufficient to deduce our opinions from correct examinations of experiments that have been already performed.

That Lavoisier was strictly correct, when he asserted that oxygen was a substance common to all acids, I firmly believe. This great genius has given us a term, merely on conjecture, which is perhaps less exceptionable, than some have imagined, viz. *muriogen*. Mr. Berthollett has also supposed the existence of this substance and has hinted at the probability of its metallic nature.

The illustrious author of all that is true in chemical science, declared, nearly twenty years ago, his belief in the existence of oxygen in all the acids, in which it had not then been demonstrated. Nor was this all; his mighty mind with heroic energy, looked forward to the day when the earths and alkalies should be found to consist of oxygen and a

metallic base. Reasoning from what he knew, he wisely conjectured that both earths and alkalies were metallic compounds. Why did Lavoisier suppose that muriatic acid contained oxygen, but on account of its known existence in other acids? and did he believe the base of the earths to be metallic? he founded his belief in their failure to unite with oxygen; knowing that a metal in its highest state of saturation with oxygen, could not readily combine with more oxygen, he rationally concluded that the earths and alkalies had a metallic base so completely saturated with oxygen, as to be incapable of combining further with this elementary substance.

What have the researches of Davy effected with regard to the alkalies? they have only confirmed the conjectures of Lavoisier and have thus proved him to be one of the most illustrious characters the world has ever produced.* Now that his anticipations respecting the earths and alkalies have been realized, shall we disdain to attempt the proof of what he has asserted of the muriatic acid? surely not. The day is yet to come when the supposed existence of oxygen in muriatic acid will be rendered certain. There was no better reason a few years past to believe the earths to be oxydes, than there is at the present day for supposing mur. acid to contain oxygen. The earths, and alkalies retained the oxygen in their composition so closely that no power was able to separate them until the aid of Galvanism was invoked.

* I am not certain that the alkalies have been decomposed; my doubts arise from having heard some ingenious remarks on this subject by W. Hembell.

And though the base of muriatic acid may be so intimately blended with oxygen as to resist our past attempts to overcome their powerful attraction for each other, we may look forward with confidence to a day not very distant, when like the earths, this acid shall be found to consist of oxygen and a metallic base. That day whenever it shall arrive, will add new tributes of respect to the memory of him, who, while opposed by his enemies on all sides, boldly withstood their attacks and proved himself a host.

It is *generally* true, that acids are, weak or powerful in their action on metals, in proportion to the force with which the oxygen of such acids is retained by their bases. Thus the aqua regia in which the oxygen is held but slightly, is the solvent of gold. Sulphuric acid will not act on this metallic body, and it is known to retain its oxygen, firmly. Muriatic acid, in a state freed of water as much as possible, will scarcely act on any of the metals, and this is owing most probably to its oxygen being so closely combined with the base, that no force has been able to separate it.

The real base of muriatic acid, we believe, will prove ultimately, metallic. For convenience, we shall name it muriogen. This is so intimately united to oxygen, as to prevent us from detecting it in a separate state; and is there any thing wonderful in this? true, we have never seen muriogen as the metallic base of muriatic acid, nor have we ever found this acid to contain oxygen.

But in these respects it is exactly on a par with potash. Who ever saw the substance potassium, or was certain of its existence until the powers of a Galvanic battery brought it to our view? or who demonstrated the existence of oxygen in the earths until by Mr. Davy's experiments it was rendered certain? Let no one deny that which by analogy is brought within the sphere of probability, and which is not far remote from certainty.

It would appear more consistent to use the terms muriatious and muriatic, in place of muriatic and oxy-muriatic acid. These we suppose to differ merely in the quantity of oxygen united to the base, as is the case with other acids.

The peculiarities of the acid when its oxygen is in excess, may and probably do arise from the nature of the base. This is by no means repugnant to reason. Potassium by union with oxygen, is said to form the peculiar substance called alkali. But it is possible that it may be converted into an acid by a still larger combination with oxygen.

Phosphorus has probably a metallic base united to oxygen, constituting an oxide. By combination with oxygen, it may be changed into an acid, the phosphoric.

Thus. in conformity with the remarks now offered, the compound nature of oxy-mur. acid is quite reconcileable; and to a candid inquirer, the causes here assigned for the peculiarities of oxy-mur. acid will appear to be founded at least in philosophy, if not in fact.

But it may be proper to say something further on the supposed decomposition of muriatic acid into oxy-muriatic acid and hydrogen gasses. It is more than probable that water and not the acid was decomposed. The hydrogen of the water might thus be evolved, while oxy-muriatic acid might be formed by the union of the disengaged oxygen gas with the muriatic acid.

To admit Mr. Davy's speculations to be correct we must seek for a new rationale to explain the production of oxy-muriatic acid from oxide of manganese, mur. soda and sulph. acid. If these speculations be indeed well founded, I can see no advantage in the oxide of manganese in this process; for not only the mur. soda must be decomposed, but also its muriatic acid, The muriatic acid being thus decomposed, would give rise, according to Davy, to the oxy-muriatic acid; and the only possible use of the oxide of manganese, must be to combine with the hydrogen of the muriatic acid, to form water.

Such a rationale in the room of one that is simple, and probably correct, appears extremely ridiculous. The production of oxy-muriatic acid from the sources just mentioned, is a sufficient ground for rejecting the hypotheses of Mr. Davy on the nature of muriatic acid. Nature does not protract her course in the accomplishment of an end that can be attained by a direct route. And there is no necessity for complicating phenomena that are in themselves perfectly simple.

A few more remarks, and I shall close. Mr. Berthollet, after repeated experiments, has procured the mercurial salt, calomel, by passing a quantity of oxy-muriatic acid gas over mercury. The plain, common mode of explaining this process, is founded on the compound nature of oxy-muriatic acid. That is to say, the oxy-muriatic acid gas was decomposed, its oxygen uniting to the mercury, forming an oxide, which, in combination with the muriatic acid, formed a muriate. But what says Mr. Davy? why, that hydrogen must have combined with the oxy-muriatic acid, to form muriatic acid. But let us examine this point a little, and that too with Mr. Davy's own scale.

Whence, then I ask, is derived the hydrogen to combine with the oxy-muriatic acid gas? is it from the *mercury?* how can this be, since we are told that the very circumstance of *its* supposed incombustibility, depends on its want of hydrogen. All the industry of Mr. Davy has not been able to demonstrate the existence of hydrogen in this metal, and it is only by analogy, that he is able to render it even probable.

But it is said that oxy-muriatic acid gas, like all other gases, holds in solution, a large quantity of water, which, by decomposition, would furnish hydrogen.

This, to be sure, if hydrogen must be obtained, is as good a mode for procuring it, as we can imagine. But perhaps this last resource, as we

hope to make apparent, will not be able to furnish the necessary quantity.

No man will pretend to say, that more than one half of the bulk of oxy-muriatic acid gas, is water. Now how are we to estimate the component parts of muriatic acid, according to Mr. Davy ? it would appear from his experiments, that it consists of about 9 or 11 parts hydrogen in 20, the remainder being oxy-muriatic acid. So that in order to form muriatic acid, from oxy-muriatic gas, we must add to the latter nearly an equal quantity of hydrogen. Well, let us suppose that Mr. Berthollet added to 1 part of mercury, 20 parts of oxy-muriatic acid gas, imagine also that this oxy-muriatic acid gas contained 10 parts of water. From this not more than 2 parts of hydrogen could be obtained by decomposition, and we are expressly told, that no combination of hydrogen and oxy-muriatic acid gas can take place, except they are presented to each other in quantities nearly equal. But suppose that oxy-muriatic acid gas should not contain $\frac{1}{4}$ th its bulk of water, the case would then be much worse for our opponents. The subject however is easy of comprehension, and the fallacy of the ancient conjecture, so warmly received in the present day, must be obvious. Delusive experiments, by means of an agent, whose real mode of operation is not known to us, may have rendered somewhat probable, the simple nature of oxy-muriatic acid. But errors like these, however masked they may be for a moment, soon vanish before the full blaze of truth. Let us, like Lavoisier, reason be-

fore we decide; for although the senses may sometimes induce us to concede to the truth of a seeming fact, yet even that evidence should not be paramount to reason.

OF COMBUSTION.

THERE is no theory on this subject which I can entirely embrace.

Lavoisier defines combustion to be the decomposition of oxygen gas by a combustible body. But I am of opinion that in every case in which oxygen combines chemically with another body, the process of combustion is essentially carried on. That is, heat and light are evolved, and the body is changed. The process may be slow or rapid; the heat and light may be evident or they may fail to be cognisable to the senses; but this is no objection to my position.

In the formation of every oxide, a portion of caloric must necessarily escape, yet oxides are often produced without any sensible evolution of heat.

That light, as well as heat, is a constituent of all bodies cannot be disproved. Of course, if this be true, no existing theory respecting the production of light and heat in combustion, can be correct.

Dr. Thomson has offered a modification of La-

voisier's theory on combustion. He supposes the combustible to yield the light, while the supporter affords the heat. In fact, I am of opinion that this is more remote from truth than Lavoisier's, which by supposing light as well as heat to proceed from the supporter must admit the existence of light in oxygen gas. Light and heat I conceive to be constituents of every supporter and of every combustible.

It is said that light has not been found to exist in oxygen gas. But I ask, whence proceeds the vivid and profuse light in the rapid combustion of a small piece of iron in oxygen gas, if not chiefly from the latter? The following fact, from Parke's Chemical Catechism, seems to prove that atmospheric air contains light. Some time ago a soldier in the French army found that heat was produced by the condensation of air in an air gun sufficient to inflame a piece of spunk. This experiment has lately been repeated before the National Institute. If the air be very rapidly compressed, heat is disengaged by the first stroke of the piston. If the end of the pump be furnished with a glass lens which admits of the inside being seen, at the first stroke of the piston, a ray of *vivid brilliant* light will be perceived.

The quantity of light and heat differs in different bodies. Hence it is unphilosophical to derive all the light from one source only. I am fully persuaded that the light and heat given out in combustion, ought not to be attributed to the combus-

tible on the one hand, nor to the supporter on the other.

There are cases, undoubtedly, in which the oxygen gas affords the greater part of the light in combustion.

We have incontestible proof that oxygen gas contains light, from the effects of light on metallic oxides. It combines with the oxygen and flies off, thus effecting what is called *deoxidation.* Now we know that light and heat have similar effects on metallic oxides, that is to decompose them. When this is done by heat, it is universally admitted that the caloric enters into combination with the oxygen and flies off in the form of oxygen gas; hence caloric is said to be a component part of oxygen gas. Why not admit the same of light? the oxygen in an oxide is known to contain a portion of caloric, as is proved by Lavoisier. We effect a decomposition of the oxide, only, by an increase of this caloric. So with regard to light; there is undoubtedly a portion of light in every oxide, and it can only be by an increase of this light, that the oxygen is enabled to quit the oxide. There is probably no combination of oxygen gas with any body in which all its coleric escapes; the same may be true of light. Much more might be said to show that light is a constituent of oxygen gas, but it will be objected, that we cannot demonstrate its existence. The same is equally true of heat; we know little of either, except by their effects.

There are many cases I grant in which oxygen gas is largely consumed without evolving much

light. But there are instances, also in which oxygen gas is decomposed in abundance, without the production of much sensible heat.

I believe that in manufactories of saturnine preparations, as miniun, massicot &c. there is as certainly a combustible process carried on, as in the burning of a candle. True there is no obvious light evolved, nor is there much heat except from the fire employed in the process. The decomposition of the oxygen gas is so gradual, that heat is not derived from this source in large quantities, yet it must be evolved in some degree, and there must also be an evolution of light. But in general, where the oxygen gas is rapidly decomposed, not only heat, but light escapes in a sensible form.

Lavoisier speaking of oxygen gas and its decomposition by means of mercury and iron, says, "that in the one case, it must be considered as a kind of gradual combustion; whereas in the other viz. the combustion of iron, the process is extremely rapid and is attended with a brilliant flame." Again he says, "as calcination sometimes lasts during several days, the disengagement of light and caloric, spread out in a considerable space of time, becomes extremely small for each particular moment of the time, so as not to be perceptible."

I therefore dismiss this part of my subject, firmly persuaded, that the light in combustion, as well as the heat, proceeds partly from the supporter and partly from the combustible. And in no case, do I believe, that the light or the heat proceeds solely from either.

Milton, speaking of light, says thus :

——————————— Before the sun,
Before the heav'ns thou wert, and at the voice
Of God, as with a mantle did invest
The rising world of waters, dark and deep.

There is so much analogy between combustion and acidification, that I cannot pass over it without a few remarks.

In fact both appear to be only modifications of one grand operation, by which all the forms of matter were produced; I mean the combination of oxygen with an original base, which was probably metallic. In acidification we have a base so called and in many cases, or I should say in all cases, oxygen, which in union form an acid. In like manner in combustion, we have a base or body capable of combustion, or of decomposing under certain circumstances, oxygen gas. We have likewise oxygen gas, or something which contains it. In fact, the production of all acids, is probably by a species of combustion. When treating of the term oxygen in the remarks on acidity, I proposed to say something on the impropriety of that word as applied to chemistry in its literal sense. We have said that oxygen signifies to beget or form acid; but in some cases of combustion no acid is formed and the result is often an oxyde.

As combustion requires the presence of oxygen, without which, flame cannot be produced, there would be no propriety in calling this the principle of inflammability, if such a term were admissible.

On this subject, however, I have made some remarks in another essay.

ON THE PRODUCTION

Of Sulphuretted Hydrogen by the Action of Black Sulphuric Acid, Diluted with Water on Iron Nails. By John Manners M. D.

HAVING a short time ago occasion to procure some hydrogen gas for some experiments in which I was engaged, I poured some sulphuric acid which had been coloured black by the falling in of a cork about six months before, diluted with five or six times its weight of water upon some clean cut nails. But from the hepatic odour emitted, I perceived that the gas generated was not *hydrogen*, but *sulphuretted hydrogen*. I applied a silver piece to the tube of the syphon from which the gas was emitted, and found it immediately blackened or converted into a sulphuret. I also tested it with the nitrate of silver and acetate of lead which were also blackened.

In what manner are we to explain the rationale of this experiment? Does it not prove in the *first* place, contrary to the opinion of Dr. Thomson and some other chemical philosophers of high respectability, that the combustible matter of the cork was capable of decomposing the sulphuric acid, at the ordinary temperature of the atmosphere, and robbing it of a portion of its oxygen? There is little doubt that the combustible matter of the cork abstracted so much oxygen from the sulphuric acid as to convert it (or a part of it at least) into *sulphurous acid;* and perhaps that of a weak degree of oxygenation. That by the action of the acid upon the iron, the water was decomposed, the oxygen of the water, united with the iron and formed oxyde of iron; which was immediately dissolved by the acid, and formed sulphate of iron; while a part of the hydrogen of the water, in its nascent state, seized upon the oxygen of the sulphurous acid, and formed water; by which the *sulphurous acid* was was converted into sulphur. While the other portion of the *nascent hydrogen*, dissolved the *sulphur* to form *sulphuretted hydrogen.*

Or shall we say that the sulphuric acid when diluted with water in acting upon the iron, gave out a portion of oxygen to oxydize the iron, while the sulphuric acid thus robbed of a portion of its oxygen, seized upon the oxygen of the water to be re-oxydized, and united with the iron to form sulphate of iron: while a part of the hydrogen in its nascent state, robbed the sulphurous acid of its oxygen and formed water, while the sulphurous

acid was converted into sulphur. Another part of the hydrogen in its nascent state dissolved the sulphur to form *sulphuretted hydrogen*?

Or shall we go still further and say that the cork abstracted all the oxygen from a portion of the sulphuric acid, and converted it into sulphur. That by the action of the remaining acid & water on the iron, the hydrogen generated (as explained in one of the before mentioned ways) in its nascent state dissolved the sulphur and formed *sulphuretted hydrogen gas*?

The first of these explanations is most probably correct.

ON THE

EMISSION OF OXYGEN GAS BY PLANTS.

By GEORGE FERDINAND LEHMAN. *Vice President of the Columbian Chemical Society, Honorary member of the Medical Society of Philadelphia, &c.*

CHEMICAL philosophers, have long considered the phenomena of vegetables interesting, and worthy of inquiry. Many of the most eminent in this science have exercised their genius, and abilities in discovering the nature and powers of plants. Among the most important questions to be elucidated, was the manner in which plants afforded oxygen gas to the atmosphere. The different opinions which have been offered on this point; and the doubt which at present exists in the minds of many, repecting the real way the air is purified; are the reasons why, I make these observations.

The ingenious Dr. Ingenhousz believed that vital air, was given out by plants by elaboration. This idea, which did not satisfy the mind of Sir

Benjamin Thompson, induced him to offer a theory, in which he says that all the air which comes in contact is inhaled by plants, and while its nitrogen and carbonic acid gas afford them nourishment, the oxygen is completely set at liberty. These notions however, were considered by Fourcroy as erroneous. This celebrated man, who shines like a comet in the firmament of chemical science, has given us a simple explanation of this process. It is not however, complete. "Hitherto the art of chemistry has arrived at the knowledge of no means of decomposing water, but by combustible substances, which take from it its oxygen. We are unacquainted with any capable of attracting its hydrogen, and setting its oxygen free. It would seem however, that nature has instruments for effecting this inverse manner of decomposing water: the leaves of vegetables struck by the rays of the sun appear to decompose water, by absorbing its hydrogen, and disengaging its oxygen in the form of vital air. This we may presume to be in part, the mechanism of vegetation, of the formation of oils, and of the renovation of the atmosphere."

This assertion is justfied by experiment; but I think it highly probable that plants in their natural state, oxygenize the atmosphere both by the decomposition of water, and impure air: and although experiments say that oxygen gss is given out by them in such situations that no air could be absorbed, it only exhibits the wisdom of nature, in having resources against such necessities.

Animals confined in a vessel after a certain

time perish, in consequence of having consumed all the vital air; if we introduce the leaves of plants in the same vessel, inverted over a quantity of water after a short period animals will exist in it. As the solution of this fact is of considerable importance, I will dwell on it for a few minutes.

It is evident, that the vital air is replaced through the instrumentality of the leaves; but how is their presence necessary to its restoration? Do the leaves elaborate oxygen gas? Do they absorb the air of the vessel, and eliminate vital air? or, is it owing to the decomposition of water? It would be inconsistent with sound reason, to suppose that a few leaves could generate enough oxygen gas by elaboration to fill a vessel and support several animals. If this were the case, a few trees would certainly be sufficient to purify a whole city.

It is replenished with oxygen gas, by the decomposition of the water, and impure air.

If Thompson's theory of itself were correct, as animals never exhale the whole of the oxygen taken into the lungs during inspiration, this *gas* would gradually be decreasing and universal death would be the consequence.

Are there any reasons, besides the improbability of the above solutions, which render it just, for us to suppose that the oxygen gas in the vessel, is owing to the separation of the constituent parts of the water, and impure air? The decomposition of this fluid is obvious from its bulk being diminished, in proportion to the quantity of gas eliimnated, and that the foul air of the vessel is also changed,

is evident from oxygen gas assuming its place. In order to satisfy myself more particularly on this point, I placed a mouse under a tumbler inverted over water. After the oxygen of the air was all inhaled it died. In this state, filled with impure air, I introduced under the glass a few leaves of aurantum (orange) over a given portion of water, and exposed it to the sun. In six hours, I placed another mouse in the vessel, and it lived. I performed this experiment a second time without the mouse, and satisfied myself that the vessel contained oxygen gas. Upon examining the water, it had diminished. Aquatic plants, or those which grow best in moist and wet situations afford more oxygen gas, than those congenial to dry places. This cannot be solved unless we admit, that as more water is in the aquatic plants, more oxygen must of course be rejected. This has been confirmed by Priestley, and Archer proves that the *Salix Viminalis* affords more vital air from a moist place (which he observes is well suited to it) than the same kind of plant, does from a dry situation. Oxygen gas will be given out by plants when no water apparently is present. This must occur from the decomposition of impure air, and moisture of the atmosphere; for an air perfectly dry would be fatal to life.

Reflecting on what has been said, we cannot but admire the simplicity and uniformity, which pervades nature; here is one of her most useful processes, animals, and plants mutually supporting each other.

ANALYSIS

OF

MALACHITE, OR GREEN CARBONATE OF COPPER OF PERKIOMING, PENNSYLVANIA.

By Thomas D. Mitchell, M. D. *Fellow of the Academy of Natural Sciences of Philadelphia, Honorary Member of the Columbian Chemical Society of Philadelphia. &c.*

THE mine at Perkioming, has long been celebrated, not only for its valuable contents, but likewise for the numerous chemical and philosophical disputes, to which it has given birth. The ores of zinc, contained in it, have excited more attention, than any other of its contents, and have been the subjects of frequent analysis.

The green carbonate of copper is found at the same mine in large quantities, and no doubt, could be worked to great advantage. This, I believe, has never been analysed, or if examined, the analysis has never been published. I have, therefore,

thought it of some importance to investigate its contents. For this purpose, I pulverized ℥ij of the ore, and subjected it to the action of a mixture of ℥i of sulphuric acid, and ℥ij of water. These being put into a bottle, the whole was accurately weighed, for the purpose of ascertaining the quantity of carbonic acid in the ore, according to the mode pursued by Accum. The action having ceased, the bottle was again weighed, when it was found to have lost 30 grains. I then poured the contents of the bottle on a filter, and the residuum amounted to 68 grains.

This residuum consisted of Quartz and siliceous earth.

Into the fluid, I then immersed pieces of polished iron and obtained a precipitate of the brown oxide of copper, amounting to 15 grains.

Thus it appears from the experiments made, that the green carbonate, weighing 120 grs.

contained of carbonic acid	30 grs.	
quartz and siliceous earth	68 grs.	
brown oxyde of copper	15 grs. =	113 grs.
Loss in the process . .		7 grs.

THOUGHTS

On the Expediency of Changing parts of the Chemical Nomenclature. By FRANKLIN BACHE, *Vice President of the Columbian Chemical Society.*

IT is well known that the nitrous oxide is capable of uniting with the alkalies, and forming peculiar salts, which Mr. Davy has proposed to call *nitroxis*, but which are generally known by the name *azotites*; this latter term we conceive to be in every point of view, exceptionable, and a departure from the original simple plan of our Chemical nomenclature.

When but two oxides of nitrogen were known, which were capable of forming salts, no greater extension of the Lavoisierian plan was necessary than to afford appropriate terms for the salts of those two oxides; they were distinguished by the terms *nitrous* and *nitric* acids, and their salts by the words *nitrites* and *nitrates;* at that time, it was not anticipated that there was yet another ox-

ide of nitrogen capable of forming salts; but as soon as it was discovered, a name should have been formed for it, on the original principle, that is, by another variation in the same name. Thus we should propose to call this oxide which has been lately discovered capable of forming salts, instead of nitrous oxide *nitral acid*, and its salts *nitrotes* instead of azotites.

But as the *nitric oxide* has not heretofore been rendered capable of forming salts, it remains a question, whether it would be expedient to change that term also: upon the whole, we think it would; since it would be the means of rendering the names of the oxides of azote uniform, as it respects the manner of their formation. Here, therefore we should propose the term *nitril acid*, instead of nitric oxide. According to this plan then we should have.

1. The nitral acid, forming salts called nitrotes, instead of nitrous oxide, or oxide of azote, forming salts called azotites.

2. The nitril acid, now called nitric oxide, the salts of which (should it be discovered capable of forming them) might be called nitrutes.

3. The nitrous acid forming nitrites; and

4. The nitric acid forming nitrates.

To bring the whole matter under one view, we should have the nitral, nitril, nitrous and nitric acids, forming salts, distinguished by the names, nitrotes nitrutes nitrites and nitrates; by this arrangement we should avoid, in expressing a set of salts, the irregular term azotite.

It may here be asked, with what propriety we can give to substances, which have not all the characteristics of acidity, the name acids ; we answer, that as long as a body agrees with the class of acids in a most important particular, such as its capacity for forming salts, we may be justified in departing from the strict definition of an acid by referring it to that head, when such a departure will be attended by a simplification of the chemical nomenclature. In fact we think no evil would arise should we even define an acid to be any oxide capabable of forming salts.

Another point, in which we think the nomenclature defective, is in the names given to the muriatic acid and its combinations with oxygen, since they are formed contrary to analogy.

We shall propose to alter these terms in the following manner ; thus for muriatic acid, let us substitute the term muriatil acid, and call its salts muriutes ; for oxymuriatic acid, the term muriatous acid, calling its salts muriites, and instead of hyperoxymuriatic acid, the name muriatic acid, distinguishing its salts by the term muriates. There can be no reasonable doubt but muriatic acid contains oxygen, that consequently oxymuriatic acid contains more of that principle than muriatic acid, and that hyperoxymuriatic acid contains more than either. Under this view of the subject the above arrangement cannot be objected to.

We have yet to mention another instance in which the principles of the simple Lavoisierian nomenclature have not been applied, at least not ve-

ry happily; we mean in naming the salts formed by the super sulphureted and sulphureted hydrogen gases; these have been called hydrogureted sulphurets and hydrosulphurets; the impropriety of these terms was obvious to Mr. Kirwan; and we have to regret that his substitution of hepar for the former, and hepatule for the latter, proved him less able to correct, than to detect error. As these substances no doubt contain oxygen, (for we in vain attempt to refuse the evidence of analogy) which, should it be proved by experiment, it would necessarily follow that supersulphureted hydrogen would be that substance which is combined with the less dose of oxygen, since its only difference from sulphureted hydrogen is an addition of sulphur; it is on this account we should wish to call the supersulphureted hydrogen, the hydro-sulphurous acid, and its salts hydrosulphites; while the sulphureted hydrogen, would be more properly named hydrosulphuric acid, and its salts hydrosulphates. These alterations we feel confident will not be objected to when it is considered what a number of terms have been adopted to express these substances without any attention to analogy: we should hardly dare to mention the various names adopted by Chenevix and Berthollet; in short we are surprised that any Chemist should adopt the termination *uret* to express a salt, that being the regular manner for expressing combinations with the simple combustibles.

But we have still a more material objection to the chemical nomenclature, as it now stands, which to understand, we must premise a few observations.

That no acid will combine with any metal, unless it be previously oxidated is a well ascertained fact; it is further known that a great majority of the metals are susceptible of several degrees of oxidation; to obviate the difficulties which this circumstance might otherwise have created, Dr. Thomson has very judiciously proposed to distinguish them, not by their colour (which circumstance is variable in the same oxide) but by prefixing to the word oxide the particles *pro*, *deu*, *tri* or *per*, according to the degree of oxidation, *pro* being prefixed to an oxide oxidated to a minimum, and *per* to one oxidated to a maximum: but we are surprised that Dr. Thomson has not extended his simplification of the nomenclature, to distinguish the salts formed by the different oxides of the same metal: thus two oxides of mercury are found capable of forming salts with muriatic acid; that salt which contains the oxide, oxidated to a maximum is called the oxymuriate of mercury by Dr. Thomson, that is, a combination of the muriatic acid with the peroxide of mercury; but we would ask, is this properly named? if so, what shall we call the salt formed by the combination of the oxymuriatic acid with any oxide of mercury, should it ever be formed, shall we call this an oxymuriate also? this would be confounding a variety of muriated mercury with a distinct species of salts, the oxy-

muriates; thus to admit such a terminology would be to create utter confusion.

To avoid these difficulties we should propose the following plan; let the different salts of the various oxides of the same metal, be distinguished as the oxides themselves, that is, by prefixing the same syllables. According to this plan, instead of calling corrosive sublimate an oxymuriate of mercury, let it be known by the name of the *permuriate* of mercury; the per prefixed, indicating it to be a muriate of the peroxide of mercury. On the other hand calomel would be properly called *promuriate* of mercury, because it is a combination of the protoxide of mercury with muriatic acid; certainly by a person not well versed in chemistry the oxymuriate of mercury would be supposed a combination of the oxymuriatic acid with some oxide of mercury, whereas no such salt exists.

Should other oxides, besides the peroxides and protoxides, be discovered capable of forming salts; then according to the oxide they contain, let them be distinguished by the appropriate particles of that oxide.

We might here object to Dr. Thomson's application of the particle per to oxides at a maximum of oxidation, without regarding whether such a maximum constitutes them acids or not; thus he calls the arsenic acid the peroxide of arsenic; we should however rather apply this prefix to those oxides which condense the greatest quantity of oxygen without becoming acids: in short we could wish to consider the word oxide as a generic term, in-

cluding under it oxides (properly so called) acids and alkalies.

We might here contend for the expediency of affixing determinate names to a set of bodies which are in every point of view interesting, we mean the compounds formed by the earths and metallic oxides with the alkalies; to these substances no definite names have been given; The combination of the peroxide of copper with ammonia has been called both the ammoniate of copper and the enprate of ammonia. The necessity of having some precise rule for forming names for these bodies must be obvious; we should propose that all these combinations should be expressed by changing the termination of the *alkali* into *ate*; thus the fulminating of silver, a combination between the peroxide of silver and ammonia, might be called the ammoniate silver; Nor will we be accused of a want of precision for omitting the word oxide in the name, since metals equally refuse to combine with alkalies as with acids, unless previously oxidated. Use might also be made here of Dr. Thomson's particles; since some alkalies dissolve the protoxide while they refuse to combine with the peroxide of the same metal.

As connected with the subject we think proper here to mention what we conceive a misapplication of terms in Dr. Thomson's work on chemistry. Under the head of oxymuriate of iron he has the following words.

"When a current of oxymuriatic acid is made to pass through water having the red oxide of iron

diffused through it, the oxide is dissolved though with considerable difficulty; But Mr. Chenevix to whom we are indebted for this experiment has not examined the properties of the hyperoxymuriate which must be formed during the process."

Now we cannot perceive any good reason for believing an hyperoxymuriate would be formed; in short under no view of the subject would it be possible. It certainly cannot mean that, since a muriate when it contains a peroxide is called an oxymuriate, therefore an oxymuriate when it contains a peroxide must be called a hyperoxymuriate; in fact it would be needless to conjecture what is meant.

We make no hesitation in declaring as our belief that this substance which Dr. Thomson has called an hyperoxymuriate of iron is nothing but the oxymuriate of the peroxide of iron; or rather the peroxymuriate of iron,; on the other hand, should the proposed alteration of the names of the muriatic acid and its combinations with oxygen be adopted, it should be called the permuriite of iron, that is, a combination of the muriatous acid with the peroxyde of iron.

We heartily wish the society would take into consideration these misapplications of terms; more especially as they occur in a class of bodies becoming every day more and more important; in fact every end intended by this paper will be fulfilled if we have demonstrated some irregularities in the present nomenclature, although the foregoing plan for obviating them should not meet your entire approbation.

REMARKS

ON PUTREFACTION.

By THOMAS D. MITCHELL M. D. *Fellow of the Academy of Natural Sciences of Philadelphia. Honorary member of the Columbian Chemical Society of Philadelphia, &c.*

When an animal or vegetable loses what has been called its principle of vitality, it necessarily takes on a new form. This change is effected by a process called *putrefaction*, which is nothing more nor less than a complete analysis of animal and vegetable matter. In order that this analysis should take place, several circumstances must concur. It has been ascertained that matter, whether vegetable or animal does not putrefy, if kept in a running stream of water at a considerable depth from the surface. If placed in a pond of stagnant water, which barely covers the surface of the ground, putrefaction then ensues with rapidity.

Here, moisture, one of the necessary attendants on putrefaction is afforded. But even this is not sufficient, alone, to excite the putrefactive process, since if vegetable or animal matter were placed under these circumstances, putrefaction could not take place, if the temperature of the atmosphere were much reduced. The materials employed in fermentation, would not be subjected to that intestine motion, which indicates decomposition, if the necessary degree of heat were not afforded. And it is also evident, that if too large a proportion of water be employed, fermentation will not ensue any more than if all the ingredients were combined in a dry state. A heat between 60° and 70° is said to be necessary to keep up the process of fermentation. It is certain that syrups tend to decomposition in warm weather unless they contain a great excess of sugar, so as nearly to obviate all moisture. In the putrefation of vegetables, all their hydrogen is dissipated, while their oxygen and carbon unite, forming carbonic acid, while a quantity of earthy matter with charcoal and iron remains. Somewhat different is the result of the decomposition of animal bodies. They contain one element, remarkably favourable to putrefaction, viz. azote or nitrogen which, with the hydrogen forms ammonia; carbonic acid gas is also evolved, with carbonated hydrogen, phosphorated and sulphurated hydrogen. To the latter, must be ascribed the peculiar foetor attendant on the putrefaction of animal substances.

The same results attend the decomposition of animal and vegetable matter, both in the hands of nature and art, and the same is effected by the natural functions of animals, as we perceive in decomposition in any other way. For example, we know the changes which ensue if animals or vegetables be exposed to a certain degree of heat and moisture; we perceive the very same effects produced by processes which are carried on in the laboratories of animals. Their foeces exhibit the decomposition of matter, the peculiar odour they possess gives a similar fœtor, and in both instances, we mark the same chain of events. However different the qualities of animal and vegetable matter may be, it is certain, that the same causes will produce similar effects on both, and the same agents will alike retard their decomposition. It is therefore taken as granted, that heat unaided by moisture or moisture independent of heat, cannot effect the putrefaction of bodies. And we naturally infer from such premises, that when both these causes exist putrefaction might be prevented, by such means as obviate either of them; that is, if putrefaction has not already commenced.

The substances which have been employed to prevent putrefaction, have been called antiseptics. Their mode of operation has never been fully unfolded, nor do we with certainty, know that the rationale of their action can be easily shown. But it becomes every one, who feels interested in natural phenomena, to investigate with care, and, if possible, to trace effects to their causes.

I do not wish to enter into a long detail of all the antiseptics employed by art to obviate putrefaction; this is foreign to my purpose. I intend particularly to inquire into some things which have always been deemed of minor importance. Because I am well convinced that it is not in the vast and stupendous objects of creation, that the grandeur of Nature is displayed, but that it often shines more conspicuously in humbler spheres.

I believe no one has ever decided by actual investigation, the philosophic use of muriate of soda in preventing putrefaction. And although the employment of nitrate of potash in the preservation of meat, is a practice of antiquity, I do not know that any thing satisfactory, has ever been adduced on this subject.

There is not a good housewife in creation who does not know the great importance of common salt in preserving meat in a sound state. Unaided by theory they confirm the correctness of their practice by long experience. And there is scarcely an old woman to be found, who will not tell you that nitre gives a red colour to meat which it does not possess, if merely salted.

We have already stated the circumstances necessary to putrefaction. As the equilibrium subsisting between the component parts of vegetable matter, is supposed to be destroyed by an absorption of oxygen from the atmosphere, so it has been concluded that the presence of oxygen* is neces-

* It would appear however, that, oxygen which is concerned in this process (if any be concerned) is that existing in the meat either as an elementary part, or in the moisture of the meat.

sary to putrefaction. This is proved, in some measure, by the fact, that meat somewhat tainted, is restored to a state of purity by means of charcoal.

Here it is natural to suppose, that by a play of affinities, carbonic acid is formed and thus the putrefactive agency of oxygen destroyed. We know too, that in the economy of domestic life, great care is always taken to preserve meat tubs in such a state, as to exclude air. But neither the air nor the charcoal are antiseptics. The former is itself a septic and the latter can do nothing more than obviate the bad effects of putrefaction, after it has taken place. Very different, of course, must be the action of muriate of soda; we must remember in the first place, that the ultimate object in view in the pickling of meat, is to preserve its constituent parts in that state of equilibrium, in which no chemical action can take place between them. This being the case, it would seem to militate against the supposition, that common salt acts chemically on the ultimate particles of animal matters, to prevent putrefaction.

That some kind of chemical action is necesssary cannot be, for a moment, disputed. But if that chemical action were of such a nature, as to operate on the ultimate particles of the matter, which it is designed to preserve in a sound state, we see no reason, why such action might not effect the destruction of that very equilibrium we sought to preserve, and thus effect a new mode of existence in the matter itself.

As I have determined to submit my conjectures to the scrutinizing test of experiment, I shall now offer a few more desultory propositions; and

1st. Does not the action of muriate of soda in preserving meat, depend on the degree of cold excited by its solution in water? It is known to every one, that common salt dissolved in water, produces a considerable degree of cold, and it is also know, that the addition of nitrate of potash renders the degree of cold, much more intense.

Now it has been already stated, and I think correctly that putrefaction cannot go on without the aid of heat. If this be true, and if it be also true, that by the use of these saline bodies we create the very opposite of heat, viz. cold, who is there, that does not immediately conclude that the proposition just stated, is correct? as cold cannot produce on animal and vegetable matter the effects of heat, so we infer, that it must act as a preventive of putrefaction, which is the effect of heat and moisture combined. How else can we, rationally, conceive it to act? If it did possess a chemical operation besides that of generating cold, we are not assured that it would be necessary, since we are aware, that cold, alone, retards putrefaction.

Besides we have said, that putrefaction does not take place when animal matter is inundated. In the preservation of meat it is customary to keep it covered with the saline solution. Add to all this the fact, that meat tubs are most commonly kept in cellars, which are always cooler than any other part of a house. That cold is extremely influen-

tial in preserving meat, if not the absolute cause, is shown by the fact, that the removal of tubs into hot situations, has actually been followed by the spoiling of that part of the meat next the surface, which perhaps was not quite covered with the saline solution. It is known also that animal bodies may be preserved in spirit, but if a part of the animal be uncovered, that part will undergo the putrefactive process in warm weather.

2nd. Does not the action of muriate of soda in preserving grass and other vegetable matter from putrefaction, depend on the production of cold?

I mean at this time, only to take into consideration, a practice of farmers with respect to the preservation of grass put into barns in a wet state. They know by experience, that common salt scattered through it, prevents it from moulding, or in chemical language from putrefaction. It is a fact worthy of notice, that if clover fresh cut be put into a barn or be stacked it will become so hot, as to excite, in some cases, combution itself; and from this cause, barns have actually been burnt. Hence, it is customary to turn up the clover frequently, to expose it to the action of the air. The cause of the great heat excited, must in great measure, depend on pressure, by which caloric is extricated; for in stacks and also barns, the clover being moist, is easily forced into close contact, and it is observed, that while the outside of a stack is nearly dry, the inside, if examined, will be found hot, and in a green state. It might be supposed, that in a stack exposed to the sun, the external surface would be

cool, owing to evaporation; and it may be, that the heat generated, is the effect, not only of pressure, but of partial decomposition, and possibly of the solidification of the water contained in the green clover. The latter is rendered probable by the fact, that the heat is greatest where the grass lies in contact with the earth. But to return from this digression; Is it not probable that the common salt acts by absorbing moisture from the clover, and thus generating a certain degree of cold, suffi- to prevent putrefaction? Where the salt is employed, the grass is preserved, in a sound state, becomes dry much sooner and is more palatable to horses.* It is true, that in turning up clover in which salt has been scattered, heat will be evolved, but this comes, chiefly, from that part of the hay next the earth, and it is certain, that the heat is not so great, when salt has been employed. If it does not act as we have supposed, we can imagine no other way in which it can operate. It certainly does not afford any principle of preservation, which enters the composition of the clover and we do think it natural to conclude, that it operates here as in the preservation of animal matter.

3dly. Does nitrate of potash give a red color to animal matter, or does its beneficial qualities depend on the power it possesses of generating cold, or are both true?

* The clover taken from the salt marshes on the eastern shore of Maryland does not require the use of salt for its preservation.

Before this question is decided, we must observe that meat never posseses a more florid color, than when fresh taken from the animal. This is a fact, of which no one can be ignorant. It has been supposed that nitrate of potash colors meat, by imparting to the latter its oxygen. But if this be the case, the nitric acid of the nitrate of potash must be decomposed, and nitrogen must be liberated, but we do not know that this is ever the case. In fact, it is more than probable, that if the nitric acid were decomposed, nitrate of potash, instead of being useful, would prove an actual cause of putrefaction, since by decomposition, it would afford a substance, highly favourable to this process, viz. nitrogen. Thus by attempting to solve one difficulty, another is created. If the red color depend on oxygenation, ought not the same effect to take place, on exposing salted* meat to the action of oxygen? but it will be said, that such an experiment could not be satisfactory, because it is probable that a definite quantity of oxygen is requisite, and because immersion in oxygen might induce putrefaction. But I answer, even though putrefaction be induced, yet the red color should appear previous to the occurrence of putrefaction, if it be true, that oxygen is the cause of the red color.

If the colour be more florid by the addition of nitre, which however I doubt, it must depend on

* I mean here, by salted meat, the animal substance impregnated with muriate of soda, only.

some other cause than mere oxygenation. It may be the effect of mixture as in the production of colors in other cases, the operation of which we do not well understand.

I am led to believe that nitrate of potash is useful in no other way, than by increasing the degree of cold, produced by muriate of soda. Because it is evident, that if oxygen, itself, will not produce a red color in salted meat, nitrate of potash cannot produce the effect ascribed to it, on the principle of oxygenation.

For the purpose of giving some confirmation to the above remarks, the following experiments were performed.

1st. I put a piece of fresh meat into a freezing mixture, and it was preserved sweet for several weeks.

2d. Having placed a piece of meat, impregnated with a solution of muriate of soda, in a glass vessel; I passed in a quantity of oxygen gas. This failed entirely to alter the color of the meat, it was not reddened in the least.

A FEW REMARKS

UPON THE NATURE OF THE NERVOUS INFLUENCE.

By Joel B. Sutherland, M. D. *Fellow of the Academy of Natural Sciences of Philadelphia, Honorary Member of the Columbian Chemical Society of Philadelphia. &c.*

DURING the existence of a considerable number of years, innumerable vague and visionary speculations, relative to the nature of the nervous power, agitated the minds of Philosophers, famed for their literary acquirements, in every branch of science.

Some indulged themselves with the thought, that it was a finely volatalized Æther, not cognizable to the optic sense; while others satiated, their philosophic curiosity with the idea, that it was derived solely from the vibrations of the nervous cords, giving or abstracting motion from muscular system of pleasure.

In this field of speculation, men whose talents seemed to cast the rays of truth upon every other subject, proved altogether abortive. Even the immortal Newton, who in imagination, could mark the path in which the revolving earth should move A. Franklin, the Prometheus of Columbia, who with the bold hand of philosophy, stold fire from Heaven, a Hunter, so minutely acquainted with the mechanism of the animal machine, retired from the cares of this busy world, unacquainted with the true nature of the nervous power. But although a Newton, a Franklin, and a Hunter, who shone so brilliantly among the departed sons of literature did not discover the exact nature of the vis nervea, still it does not follow, that a person of much meaner capacities, may not develop the secret in our times.

For in the days of modernity, when science shines more resplendent than heretofore; when the ingenuity of man is so great, as to follow nature through her various meandrings, and expose to the admiration of the world, secrets which many of our forefathers, were convinced would lay concealed in the womb of time forever. I say when such events as these have taken place, we must have a thousand different substances, and collateral circumstances to aid us in our scientific enquiries with which our predecessors in science were unacquainted. This then is the apology I shall offer for attempting to explain the true nature of nervous vis, a subject so long veiled in obscurity.

When the learned D. Priestley made the world acquainted with his discovery of nitrous oxid, or what was formerly called dephlogisticated nitrons gas, he never once supposed, that at a future day, this exhilerating gas, would be considered by any one, to be the great and only stimulus, that should carry on the operation of the animal system. But nevertheless, I shall attempt to predicate this opinion, upon the firm basis of correct reasoning.

This globe of which we are inhabitants, is surrounded by an invisible fluid, commonly known by the name of atmospheric air, whithout which the life of man would instantaneously cease, the heart and arteries be silenced in death, and all the beauties of creation would vanish into non-existence.

To explain the manner in which air acts upon the body, so as to become highly important to the vital operations of the system, will be the object of the subsequent remarks.

Atmospheric air is composed of about 22 parts oxygen, 77 parts nitrogen and some say 1 part of carbonic acid in the hundred. Now as soon as a sufficient quantity of this mixture is inhaled by the lungs, a general decomposition immediately commences. But with respect to this decomposition, different opinions are tenaciously observed; some suppose that the oxygen of the atmospheric air, unites with the carbone of the venous blood, and is finally eliminated from the system, in the form of carbonic acid in a state of mixture with the ni-

trogen. But of what use would nitrogen be then? they say to blunt the irritating effects of oxygen. And why? Not because nature intended her operations should be explained in such an imperfect manner; but because nitrogen, according to their theory appears to be of no other use. Thus much for one of the theories in high repute with some chemical philosophers; while others advocate an opinion, in a great measure directly opposed to the one just mentioned.

They say that the oxygen unites with iron, or something else in the venous blood, and thus produces a red colour, not negatively, as is the case in the former theory, by decarbonization, but positively, by its union with some of the substances in the blood. But if this opinion were correct I would ask, from whence does the carbonic acid originate, which is observed in every expiration?

Does it originate from the carbonic acid, which is said to be one of the ingredients in air? No it cannot. For this is insufficient to correspond with the quantity exhaled.

Therefore such an opinion as that which supposes that oxygen acts *positively* in rendering the blood fit to pass from the veins into the arteries: according to my ideas upon the subject, is not only *conceived*, but *brought forth* in error.

From what has been said, it must appear evident that the great aim of those philosophers has been to establish a use for oxygen; while nitrogen, has according to b th theories, been applied to no other use, than to blunt the irritating effects of oxygen."

But if this was the intention of nature, I would ask why did she form so large a quantity of nitrogen to inflate the lungs, in conjunction with oxygen? why did she not make oxygen less stimulating and more congenial to the pulmonary system? If her object was what they endeavour to establish, then nitrogen would not be required. Certainly Nature most commonly dresses her operations in the garb of simplicity. And why should she here make an exception to the rule? she has not. 'Tis only the imperfection of a theory, that would make it for her.

I shall now endeavour to show the intention of Nature in forming our atmosphere as has been mentioned before.

1st. That a portion of the oxygen might unite with the carbon of the venous system, and thus change venous, into arterial blood; while the remaining portion of oxygen, would unite with the nitrogen, and thus produce nitrous oxid, a stimulus so congenial to the nervous system.

But here some may remark, that there is more nitrogen than is sufficient to form the whole into nitrous oxid, or rather, that it is not in an exact proportion, viz. 63, 3 nitrogen, to 36, 7 of oxygen in a hundred. Be it so. Nevertheless a portion of nitrous oxid may be formed, which would (perhaps) be too stimulating to the nerves, while the other portion would reduce its activity. Thus we see nitrous oxid may be formed. But here some may ask, how is nitrous oxid formed, when nitrogen alone is said to be expired from the sys-

tem? I answer nitrogen alone, (or rather pure nitrogen) I believe never was expired from the lungs, but an impure gaseous oxid of nitrogen which has been mistaken for it.

That this might be the case, no one will deny when they recollect, that we have no positive test for the presence of nitrogen.

But I shall endeavour to make it more evident. If we abstract oxygen from an animal, and feed the system upon nitrogen, one of the ingredients in our atmosphere, death will be the immediate consequence, or if we reverse the practice and give oxygen alone, death will finalize the scene. Why does such an occurrence as this take place? Why does the animal live with their union and not their separation? I answer. Because no nitrous oxid can be formed, with one of those ingredients. Both are necessary for its production. Thus it appears very probable that nitrous oxid is necessary to animal existence.

Having then rendered it very probable, that the nitrous oxid is necessary to life, I shall now attempt to show that the nitrous oxid, and the nervous influence is precisely the same.

When the gaseous oxid of nitrogen (or what is now called nitrous oxid) is mingled with atmospheric air, and then received into the lungs, it generates highly pleasurable sensations: the effect it produces on the animal system are eminently distinguished from every other chemical agent. It excites every fibre into action and rouses the faculties of the mind, inducing a state of general ex-

hileration, an irresistible propensity to laughter, a rapid flow of vivid ideas, an unusual vigour and fitness for muscular exertion, in some respect respect resembling those attendant on the pleasantest period of intoxication, without any subsequent languor, depression of nervous energy, or disagreeable feeling. Now if such an effect is produced by nitrous oxid, viz. that of exerting every fibre into motion, and an increase of what we call nervous influence produces the same effect; may we not infer that the nervous inflnence, and nitrous oxid are one aud the same thing.

But here some may ask, whether opium, wine, and bark will not increase the action in the system? yes it will. But not in the same manner. When these medicines are taken into the system, they lessen the excitement and of course increase the excitability of every muscular fibre. When this has taken place, the nervous power, although no greater than before, will act with more energy and produce a greater effect. This then is the reason why opium, wine. &c. increase the pulse. Bnt here I may be asked, how I can determine that opium does not act like the nitrous oxid. I answer, 1st. Because there is no diminution of nervous energy, when we inhale the nitrous oxid. And why is there no loss or diminution of nervous energy? because the nitrous oxid is the nervous energy itself; therefore there can be no loss.

2nd. Because the functions of the system will go on without opium &c; but will not without the gasses from which nitrous oxid is formed.

I have now finished my remarks upon this subject, believing from what has been said, that the following inference may with great justness be drawn, viz. That the intention of nature in forming our atmosphere of oxygen and nitrogen was, that the gaseous oxid of nitrogen, might be formed, so as to yield nervous influence to the system of man, and thus carry on its vital operations.

CHEMICAL VIEW OF SECRETION.

By THOMAS D. MITCHELL M. D. *Fellow of the Academy of Natural Sciences of Philadelphia. Honorary member of the Columbian Chemical Society of Philadelphia, &c.*

THERE are some subjects in physiology, which from their apparent impossibility of solution, have been classed among the hidden mysteries of nature. Experiment and speculation have followed each other, but in vain; the difficulties have not been removed and we at last have been compelled to take refuge in ignorance.

The more immediate object of these remarks is to consider the phenomena of Secretion. And here as in many of the most important animal functions, the aid of chemical science shall be invoked. That science, by whose illuminating beams, the sable gloom of nature has often been removed, and the mysteries of creation exposod to the full blaze of day.

Conducted by this torch, enter with me, for a few moments, the great laboratory of man, and if the glimmerings of the taper should grow fainter and fainter and at last expire, we can only console ourselves by reflecting, that in obscurity we found the subject and in darkness left it.

I shall not attempt to fatigue the mind by offering an epitome of the many vague conjectures on this subject. Because the opinion I am about to support, might be deemed still more wild, and render the whole irksome and disgusting.

In the first place, I will state the position about to be supported, and then attempt its elucidation. The position is simply this. That the difference in the various secretions, depends chiefly on the difference in quantity of the oxygen contained in the blood, sent to the various glands.

The position is founded in great measure on the difference between venous and arterial blood. The latter, by chemical analysis is found to contain more oxygen than the former. And perhaps it may contain even more than has been supposed. With regard to this difference, it matters little whether we derive it from the lungs or from the Thoracic Duct. By the former the blood is said to receive a portion of oxygen, independent of that by which the oxide of carbon is elicited from the venous blood. By the latter viz. the thoracic duct, the blood is furnished with oxygen, because the substance, is no doubt, produced in considerable quantity by decomposition of the elementary articles containing it. Admit that the oxygen is sup-

plied from both sources, and then let us pursue the fluent blood.

In its course we shall endeavour to show, that the quantity of alkaline matter in the secretions increases as the quantity of oxygen diminishes, or that the characteristics of acid are gradually opened.

Thus for example, we shall adduce the mammary glands. These are the largest glands which are near to the centre of circulation. They are supplied by vessels of considerable size and of course carry a considerable quantity of blood in as high a state of oxygenation as we find it in any part of the system, except at the heart.

If we examine the properties of the mammary secretion, we find it to be sufficiently acid to redden the vegetable blues, and a given portion of it contains less alkaline matter than is found in secretions more remote from the centre of circulation.

It is unnecessary to take into consideration every gland or surface of secretion in the human body. I shall therefore examine the secretions of a few others which have always been considered as among the most important.

Let us for a moment consider the bile and urine, two of the most extensive secretions. Are not these calculated to illustrate the truth of a remark already made, viz. that alkalescence increases in the ratio of the distance of glands from the centre of circulation? The bile contains perhaps more alkaline matter than any other secretion, and why? I answer, by far the greater part of the blood from

which the secretion is produced, has travelled a most extensive and circuitous route before its arrival at the liver. The blood is in fact venous, and probably conveys no oxygen to the liver; moreover, part of this same blood had passed as arterial blood through the substance of the pancreas, the secretion from which contains but a very small portion of alkali.

With respect to the urine, it is found that acid properties are evinced by the introduction of a vegetable blue. This is explicable by the fact, that the emulgent blood has not to travel a 20th part of the distance allotted to the blood carried to the liver by the venæ portærum. Let it be also remembered that nearly $\frac{1}{8}$th of all the blood passes to the kidneys.

It may be said, that our mode of argument will not explain the reason why the urine should possess so much alkaline matter. I am prepared to meet this objection. Nearly every article which chemical analysis has detected in the blood is found in each of the secretions. The quantity of each article appears naturally. to be influenced by the quantity of blood carried to each gland. Now I do not contend that the sensible difference in the secretions can be accounted for, merely by the variable portions of blood sent to different glands; by no means. I maintain that this difference depends rather on the quality of the blood distributed for the purpose of secretion.

Now the blood in all situations, is found to contain certain substances, but I do maintain, that

oxygen is not found in venous as in arterial blood and that its quantity lessens constantly from the heart to the termination of the arterial system. Therefore that similar articles should exist in the different glands, is what we would naturally expect; and that a modification of these articles, so as to render their combination in one instance bland, in another bitter, is to be attributed to the variable proportion of oxygen conveyed to each gland by the circulation.

The application of my position to a general investigation of glandular secretion, will no doubt be attended with difficulty. Nor can it be expected that we should be able to obviate all obscurity, on a subject, which has been considered over and over again, without so much as reflecting a single ray of light on its nature.

The observations now made, if not in themselves satisfactory, will, it is hoped, tend to a more general attention to a subject so interesting to Physiologists.

OBSERVATIONS

Upon the effects of various gases upon the living Animal Body. By Edward Brux, *of France.*

ALTHOUGH the subject of Respiration has engaged the attention, and exercised the genius of very eminent physiologists, and although it cannot be denied that the ingenious experiments of Haller, Spallonzani, Goodwyn and others have thrown a great deal of light upon this very important function of the animal oconomy, yet notwithstanding the discovery of many important facts, the subject remains involved in much obscurity, and the experiments lately made in Europe satisfactorily prove that much remains yet to be ascertained not only with respect to the chemical change which takes place in the blood in consequence of respiration, but more especially with respect to the agents and laws of the animal oconomy by which this change is effected.

Many celebrated physiologists by observing that blood exposed to oxygen gas, or even atmospheric air, changed gradually from a deep purple to a bright scarlet on the surface exposed, and acquired the color, if not the properties of arterial blood, were induced to believe that the change effected in the lungs was perfectly similar to that which they witnessed in their experiment, and that the function of respiration consisted merely in the mechanical phenomena by which the blood is brought in contact with atmospheric air, and in the change effected by the decomposition of this gas. They considered the lungs as perfectly passive, which is certainly true with respect to the mechanical functions of respiration;* for always in contact with the parietes of the thorax in a healthy state, they dilate and collapse with them in consequence of muscular action. But they cannot be considered as chemical recipients, for from a series of experiments reported to the French Institute by Mr. Dupruytren on the subject of respiration it appears that the contact of air is not sufficient, and that it is not merely by the laws of chemical affinity that the change in the blood from venous to arterial is effected. But that it depends also upon a nervous influence which seems to be as necessary to the due performance of this function as it is to that of secretion, nutrition, &c. it is true that Richerand attributes to the lungs a vital energy. But

* Contrary to the opinion of Dumas, Brumond and others who supposed that the lungs are possessed of an innate power of motion.

it is not till lately that the cruel but very interesting experiments made by several able physiologists in Paris have at least rendered very plausible this direct influence of the brain npon the chemical phenomena of respiration. I say plausible for it seems that some of their experiments contradict each other, and before this important point be fairly ascertained, or looked upon as an axiom in physiology, many more experiments, must be performed or those already made must be reinvestigated, and more satisfactory conclusions drawn from them.

But if it be true that the function of respiration is not yet completely understood, how much more defective is our knowledge of the deleterious properties of various gases and effluvia upon the living animal body, this is a subject which has not yet been as duly attended to as its importance seems to me to require, and which offers to the physiologist experimenter, and even to the practitioner of medicine a very extensive field for inter esting and very useful investigation.

The subject of this dissertation being an enquiry into the proximate causes of asphyxia produced by the Inhalation of various gases, we will begin by first investigating the effects of a total suppression of respiration, inquire what obvious changes take place which prove the immediate cause of death, and lastly we will attempt to determine how far the inhalation of various gases seems to induce asphyxia by the deleterious properties which they possess, and how far we are to attri-

bute the various symptoms which follows their inhalation to their negative properties or incapability of oxygenising the blood.

1st. Of the changes which the blood undergoes in respiration, that which is returned to the heart by the venæ cavæ, &c. is afterwards sent into the pulmonary arteries by the contraction of the right ventricle, is heavy and of a deep purple colour. According to Richerand its temperature is only 30° of Reaumur's thermometer, and if suffered to remain still, it coagulates slowly and separates a large portion of serum. That which is brought back to the heart by the pulmonary vein, and which is conveyed into every part of the body by means of the arteries, is on the contrary of a red vermillion colour, frothy, lighter and two degrees warmer; it is also more easily coagulable and separates a smaller proportion of serum.

All the changes in the colour and properties of the blood which are so easily perceptible and which certainly depend upon the modifications arising from having been in contact with atmospheric air, have given rise to many interesting experiments and ingenious speculations. It is not indeed surprising that a subject of so great importance should have attracted the attention of physiologists, who in this as well as many other obscure points of this science have too often relinquished truth in search of proper arguments to defend and support a favourite hypothesis.

Some have supposed that the change depended merely upon the absorption of oxygen, whilst it has

been attributed by others to the absorption of this gas, and the emission of carbon. According to Richerand the blood in changing from venous into arterial, absorbes a portion of oxygen and caloric, which combining with hydrogen and carbon in every part of the body form water and an oxide of carbon; the former dilutes the venous blood, renders it more fluid than arterial, and when in the lungs exhales dissolved by the air, forming the pulmonary transpiration or exhalation; whilst the oxide of carbon by combining with a fresh supply of oxygen when under the influence of respiration constitutes the carbonic acid gas found combined with the air expired.

This theory is ingenious and seems to be the most plausible of all the various speculations upon this subject. Many physiologists have, however, even lately attempted to refute it. Dr. Bostock supposed the aqueous vapour emitted from the lungs to consist of the water poured into the blood ready formed by the thoracic duct, and Messrs. Allen and Peppy concluded from a series of experiments that no water is formed in the blood by the combination of hydrogen and oxygen, whilst Mr. Abernethy maintained that the carbonic acid is not the product of respiration, but simply exhaled from the pulmonary vessels. It would be both useless and uninteresting to spend much time in investigating the various opinions entertained by different writers upon this subject; and whatever may be the precise change which takes place in the blood when circulating through the

capillary system of the lungs the fact is certain that a change takes place, by which the blood is as it were, revivified. That such a change is necessary to the due performance of all the different functions of life, experience has given us too many melancholy proofs. Indeed as we shall see hereafter, death from submertion strangulation and suffocation is always the consequence of a total suppression of the chemical phenomena of respiration, or in other words, of an interuption of this change in the properties of the blood.

This is not, however, an universal opinion among physiologists; very far from it: this point has been the subject of much controversy and debate, and if we look over the works of different writers, we will find that they have each of them started, and attempted to establish an hypothesis of their own. Haller and other able physiologists supposed the proximate cause of asphixia to depend upon a collapsed state of the lungs which occasioned death by mechanically obstructing the circulation of the blood in this organ, and of course, through the other parts of the body; it was by this explanation (perhaps modified by succeeding writers) that physiologists accounted for the death succeeding a preternaturally protrated expiration, until the celebrated Goodwin by a sett of very ingenious experiments, proved their theory to be erroneous, and the interruption of the mechanical functions of respiration, to be only indirectly the cause of death. It is to the cessation of the chemical phenomena which must inevitably follow a privation of the

agent which supports them, that this eminent physiologist attributed the cessation of the contractions of the heart; according to his opinion, the left ventricle not finding in the unoxygenised blood a sufficient stimulus to excite it to action, ceases to perform its important function of transmitting the vital fluid to every part of the body, and life then ceases because the circulation is stoped. Goodwyn's explanation differs then from that of his predecessors in this point only, that he refers to the heart exclusively what they attributed to the state of the lungs. But although he made a step towards the truth, he did not, however, give an explanation which could stand the test of succeeding investigation, and the celebrated Bichat proved by observations and by many direct experiments made upon living animals that the circulation is not immediately stoped and that the stimulus of nervous blood may produce upon the internal surface of the left ventreil a sufficient degree of excitement to form it to contract, if not altogether with as much force at least in a very sensible manner. According to this very ingenious physiologist, life does not cease because the blood is not sent by the heart to every part of the body but because this organ transmits to them a fluid not only incapable of exciting them to action; but even possessed of deleterious properties, "it is," says he, "by circulating in the coronary arteries, and by penetrating the texture and debilitating each fibre of the heart individually, that the venous blood acts in inducing a cessation of the contractions of this organ, and

not merely by coming in contact with its internal surface. But it is not upon the heart alone that it exerts its deleterious influence; it acts upon every part of the body and destroys the function of the brain, of the voluntary muscles, of the membranes and in a word, of all the different organs through which it circulates, exactly in the same manner that it inflames the heart.

This explanation of the cessation of life is at least very ingenious, and seems to be very plausible. But supposing it to be true, a very important question remains yet to be solved. How does venous blood act upon the nerves or fibres in inducing asphyxia? is it possessed of deleterious properties from being supersaturated with hydrogen and carbon? or is it indirectly that it occasions death by being deprived of the principle emissary to the action of the different organs? * but although the retention of carbon is one of the causes of death, venous blood cannot, I think, with propriety be said to possess any deleterious properties from its being discharged with this principle for it is not on account of the quantity of carbon which it already contains that it proves injurious; But on account of its being deprived of the property of abstracting this deleterious principle from every part of the body; on the other hand, the absorption of

* We might cite the authorities of Richerand and Bichat in support of the opinion entertained by some physiologists respecting the positive noxious properties of venous blood. Richerand says "perhaps also the venous blood accumulated in every structure, affects the different organs with its blunting and cemortiferous qualities.

oxygen cannot, I think, be, as some have supposed entirely subservient to the evolution of carbon. Whether this gas is the principle of excitability or not? a question which many years ago was the subject of much controversy among hypothetical physiologists, I will leave to more able and speculative physiologists to decide, although I must confess, that from my ideas of the true meaning of the word *irritability* or *excitability*, I should be induced to think that it was a property or a quality of matter as Dr. Rush expresses himself, and not a substance.

But I think, that besides serving the important purpose of decarbonising the blood, the absorption of vital air, no doubt imparts to this fluid other properties which render it capable of exciting into action the heart and other organs.

Thus it appears that when the respiration is stopped by any mechanical means, the unoxygenised state of the blood, and the retention of carbon which must inevitably follow the interruption of oxygen to the lungs, are the proximate causes of the cessation of life. Death from submersion or strangulation of life must then be accounted for upon this principle. Since from the experiments of Kite, Coleman, Goodwyn and others, it is very satisfactorily proved that the cessation of life from the above causes depends upon the interruption of the mechanical phenomena of respiration.

It is also in this way that the inspiration of various gases leads to induce asphyxia. It was first ascertained by Mr. Lavoisier, and Mr. Seguin

that hydrogen or nitrogen when received into the lungs in a stste of purity, are altogether passive and destroy life only by preventing the access of oxygen. The truth of this interesting fact has since been confirmed by the experiments of Mr. Davy and other chemists; carbonic acid gas is also supposed by Richerand to act in the same way in inducing asphyxia, and a member of this society has lately attempted to account for the negative properties of this gas, though containing a certain quantity of oxygen, by observing, that the matter which is to be thrown off from the lungs, being similar to that which forms the base of carbonic acid gas, the oxygen will not quit the latter to join the former; from this he concludes that no air can be respired for any length of time, which does not contain a portion of oxygen united to something different from the matter to be evolved from the lungs; although his conclusion is correct as far as it relates to carbonic acid gas, it is not strictly true taken in a more general point of view, and supposing his ideas with respect to the manner in which the inspiration of this gas induces asphyxia to be perfectly just, his sentence, by a little alteration may be rendered more generally and more strictly correct. He ought to have said, no air can be respired for any length of time with safety, which does not contain a portion of oxygen united to something for which it has less affinity than for the matter to be evolved from the lungs" for on the other hand oxygen gas may be, and is in reality contained with aqueous vapour, which matter is

certainly evolved from the lungs, and yet we do not find that by this combination it is deprived of the property of oxygenising the blood even perhaps in the smallest degree, and on the other we know that oxygen though not contained with carbon in the form of carbonic acid gas may notwithstanding be rendered unfit for respiration by being united to something for which it has a srronger affinity than for carbon, or the matter to be evolved from the lungs.

But although we might upon this principle satisfactorily account for death taking place, when an animal is confined in an atmosphere of carbonic acid gas, yet from various circumstances, I feel confident in my own mind that succeeding experiments will prove the positive deleterious properties of this gas when received into the lungs. The impossibility of respiring it in a state of purity, was proved by the experiments of the unfortunate Pilatre de Rosier, and those of Mr. Davy. This celebrated chemist tells us that when he attempted to breathe a mixture of two parts of common air and three of carbonic acid, it produced a burning cessation at the top of the uvula, and stimulated the Epiglotis so as to close it spasmodically upon the glotis, and thus in repeated trials to prevent him from taking a single particle of it into his lungs even by the most powerful voluntary efforts. This want of success however, only served to increase, instead of diminishing his intrepidity; he mixed three quarts of carbonic acid with seven of common air, and finding that it was

respirable, he breathed it near a minute. At the time, says he, it produced a light degree of giddiness and an indication to sleep. These effects however, very rapidly disappeared. Dr. Botock, it seems attributed also to carbonic acid noxious properties, for in speaking of the effects of azote when taken into the lungs, he says, it produced a sense of suffocation more speedily than hydrogen, but it appears that the gas employed in the experiment contained a quantity of carbonic acid to which we may with great probability ascribe its noxious properties; carbonic acid, says Mr. Davy, possesses no action on arterial blood, hence perhaps its slight effects when breathed mingled with large quantities of common air; its effects are very marked upon venous blood! if it were thrown forcibly into the lungs of animals, the momentary application of it to the pulmonary venous blood would probably destroy life; many other authorities might be quoted in support of the opinion entertained by many physiologists respecting the noxious properties of this gas; they seem all to agree in attributing to it when sufficiently diluted with common air a certain sedative property, in consequence of which it destroys life apparently by rendering the muscular fibre irritable, without producing any previous excitement. The effects of hydrocarbonate gas seem to be very much alike those of carbonic acid when taken into the lungs, though from the observations of Mr. Watts and the experiments of Dr. Beddoes, it seems probable that the former is perhaps more highly deleterious. Its ef-

fect are thus described by Dr. Bostock; "if the carbonated hydrogen gas," says he, "be inspired in an undiluted state, it is followed by instant death, and when employed in small quantity only, mixed with atmospheric air as with oxigen, if it be used for any length of time, it induces vertigo dimness of sight, convulsions, loss of sensation; and in short every symptom of approaching dissolution. "The noxious influence of this gas," he adds, "I conceive with more propriety referable to its action upon which it produces directly sedative effects; we might to this short analysis of the effects of hydrocarbonated gas, add a quotation of the interesting and beautiful description by Mr Davy of the terrible effects of this gas upon himself. But as it is too long I will content myself in quoting a part of his last paragraph in which he says, "I have been minute in the account of this experiment because it proves, that hydrocarbonate acts as a sedative, i.e that it produces diminution of vital action, and debility, without previously exciting."

From these interesting accounts of the effects of carbonic acid and hydrocarbonate gas, to which, if necessary, many more might be added; we may safely conclude, that, if some gasses seem to possess only negative properties when received into the lungs in a state of purity, and to induce asphyxia only by preventing the access of oxygen, there are others among the respirable gasses or those which may either in a diluted or pure state be received into the lungs, which possess positive and highly deleterious properties, and which al-

though they might produce, like the former death, their incapability of oxygenising and decarbonising the blood, nevertheless occasion such traces of symptoms as cannot be refered to a state of the blood, and leave in the mind of a faithful observer no room to doubt of their noxious qualities. But it may be asked, how do such gases when inspired act in destroying life? this is a question of the greatest importance either in a practical or physiological point of view, and which requires the performance of many experiments and the careful investigation of very ingenious and perhaps speculative physiologists before it be fully and satisfactorily explained. But little, I believe, can be said upon this important subject at present; what I shall say upon it may be comprised in a few observations among the various gases possessed of deleterious properties, some seem to act by their corrosive nature, and to create inflammation and gangrene in the substance of the lungs themselves: among these may be ranked various concentrated mineral acids in the state of vapour, an atmosphere loaded with various metallic substances of a highly stimulating nature such as arsenic, corrosive sublimate &c. in a very subtile powder; among these are also placed by Richerand the exhalations of prisons or burial-places; in asphyxia induced by these effluvia, says this great physiologist, the lungs are often covered with black and gangreous spots, and death seems to be the effect of a poison, so much the more active as its parts being extremely divided and reduced to a gaseous state are more penetra-

ting and affect the nervous and sensible surface of the pulmonary organ throughout its whole extent. Many other gases however do not leave after death any marks of a loss of organisation in those parts which we know to have been the medium through which the injury had been sustained and as we have already attempted to prove that all gases are not deleterious merely by a relative action, as some have supposed, we must explain the phenomena of asphyxia in these cases upon other principles. Do not the interesting experiments of Mr. Davy upon nitrous oxide throw some light upon this important subject? and is it not probable that this gas, like oxygen is absorbed by the blood, and being sent by the heart to the brain and other organs destroys life as Mr. Davy expresses himself by producing the highest possible excitement.

I cannot however suppose carbonic acid and many other gases to be absorbed, and their effects may be I think soon satisfactorily accounted for in another way; the experiments of Mr. Dupreytren Mr. Legallois and many other able physiologists in Paris which tend to prove that instant death is always the consequence of a division of those branches of the eigth pair which supply the pulmonary organs, and consequently that a nervous influence is necessary to the due performance of the important function of respiration, seem to us to be very interesting and well worthy of the attention and careful investigation of physiologists who are capable of handling such a difficult subject; do not those experiments tend to throw some light upon the

deleterious effects of gasses? and since the different organs, but more especially the stomach is susceptible of being excited by so many various stimuli, and of affecting by sympathy the brain and indeed the whole system, why should not the lungs possessed also of a nervous influence be equally susceptible of being variously excited by the stimulus of various gases, and of sympathising with the other organs? may not carbonic acid and hydrocarbonate gases, to which we have seen Mr. Davy and Dr. Bostock (influenced only by the symptoms which they witnessed,) ascribe directly sedative properties, may not these gases, I say, act upon the pulmonary system exactly in the same way that a large dose of opium acts upon the alimentary canal? But let us not seek to penetrate farther than experience can guide us; and although the advice of our experienced professor Dr. Rush of speculating upon physiological subjects may be the source of the most important discoveries, yet I believe the following observation of Dr. Bostock to be founded upon the truth. "Physiologists," says he, "have, in general, been more inclined to form hypothesis than to execute experiments, and it has necessarily ensued from this unfortunate propensity, that their science has advanced more slowly than perhaps any other department of natural philosophy."

ANALYSIS

OF

PROFESSOR COXE'S ESSAY ON COMBUSTION AND ACIDIFICATION.

By THOMAS D. MITCHELL M. D. *Fellow of the Academy of Natural Sciences of Philadelphia. Honorary member of the Columbian Chemical Society of Philadelphia, &c.*

I have long thought and still believe that matter was at one period quiescent, and that its sudden transformation into forms innumerable, was the effect of a grand and general combustion. Perhaps the electric fluid which I consider as one of the pristine parts of creation, was the grand agent in effecting this process. Would it not seem that combustion had preceeded the various products of our globe, when we recollect what are the effects of combustion in our own hands. It produces an acid, an oxyde or water. In all

creation there is not perhaps an atom which does not belong to one of these classes.

Metals may be called simple, but I rather believe them to be compounds. and that they are never found free from oxidation in some degree. View then this grand process converting all matter into forms which must be classed either among acids, oxydes, or water. The combinations of saline matters, the vast and boundless current meandering to the world's extremes, are placed in one view before us, and thus in earlier days as at present, we behold combustion, as the grand agent of decomposition and recomposition. This view is simple, but perhaps not the less correct.

Every theory therefore of the phenomena of combustion that discords with the original and universal process must be founded in error.

In considering this essay, I shall begin by analysing the first objection to the Lavoiserian system of chemistry. The author adduces from Dr. Thomson the following experiment, viz. "If 8 parts by weight of copper filings, mixed with 3 parts of the flowers of sulphur, be put into a glass receiver, and placed upon burning coals, the mixture first melts, then a kind of explosion takes place; it becomes red hot; aud when taken from the fire, continues to glow for some time like a live coal. If we now examine it we find it converted into sulphuret of copper. This experiment succeeds equally well however fine and dry the sulphur and copper be; and whatever air be present in the glass vessel, whether common air, oxygen gas,

hydrogen or azotic air." Now what is to be proved by this experiment? Why, that combustion may go on without oxygen; and does it prove this? far, very far from it. To say the least of this experiment, it is certainly improbable. I will grant for a moment that something like combustion might go on even in contact with azotic gas; but will any man of plain common sense, credit the assertion, that azotic gas, is as favourable to combustion as oxygen gas? The flowers of sulphur as every man knows, contain a portion of acid, and it follows that if combustion did ensue in azotic gas, it was dependant on the acid existing in the flowers of sulphur. But even admitting that we were unable to explain this supposed anomale; is the whole system to be blasted by one solitary objection? why does the author of the essay pass over this experiment and attempt neither its proof nor disproof? he was either, not in quest of the truth, or felt his inability to solve this apparent difficulty.

I must beg leave to correct the professor before I go further, with regard to Mr. Lavoisier. He misunderstands that celebrated and illustrious author. Speaking of light and flame, he says, "Lavoisier supposed them to constitute a part of oxygen in its gaseous state, and that they separated from it, when it became fixed in the combustible." Now, Mr. Lavoisier did not mean that all the light and heat separated; on the contrary, he has proved by experiment, of which any man may satisfy him-

self, by reading the "Elements," that a *part only* of them was separated.

But I come to another point, and here let the error first corrected, be kept in view. "The caloric of atmospheric oxygen escapes," says the professor, "when it loses its gaseous form or becomes united to the burning body, as caloric escapes from the latent to a free state, when water in the state of vapour condenses to the fluid form. "But how," continues he, "can we reconcile this with the attendant phenomena of the combustion of gun powder, in which numerous fluids are produced suddenly from a state of previous solidity."

How weak, how futile is this argument! will men ever search the truth in scientific inquiry, or are they always to grovel in error? Before considering the phenomena of the combustion of gunpowder, I will oppose one question to the argument made use of. The professor thinks that the antiphlogistic system is imperfect, because it cannot account for the fact that a solid body should suddenly be converted into a large volume of elastic matter. Why then, I ask, is one drop of water, capable of being converted into a form more fluid and with its bulk increased many hundred times? true, it may be said, we do it by the force of caloric. And pray, have we not the same force in gun-powder? or rather let me ask; did Lavoisier toil in experimental inquiry to prove this point, by irresistible evidence twenty years ago, and has it not yet reached the ear of the professor? it can scarcely be so.

I wish not to force the judgement of any man, but to set the truth before his eyes In page 158 of Lavoisier on Nitric Acid we read thus; "We have before seen that in the state of oxygen gas, it contained at least 66, 66667, of caloric; therefore it follows that in combining with azot to form nitric acid, it only loses 7,94502." This is the result of experimental research, and proves that a given quantity of oxygen gas containing 66 parts of caloric, lost only 7, when it combined with nitrogen to form nitric acid. "This enormous quantity of caloric," says the great Lavoisier, "retained by oxygen in its combination into nitric acid, explains the cause of the great disengagement of caloric during deflagrations of nitre, or more strictly speaking upon all occasions of the decomposition of nitric acid.

"But," says the professor, "whence can arise the caloric so suddenly reqnired to retain them in a gaseous form? It will scarcely be said to come from the solid materials since this would militate against his own opinion." No man could have understood the writings of Lavoisier, while writing in such a style as this, since it is evident that the subject has been fully explained.

With regard to the Professor's remarks on Dr. Thomson's theory of combustion, I would only say as I have done in another place, that it is but a modification of Lavoisier's theory, and perhaps less correct than the latter. I have also said something of the inflammable principle, so called, but I have still more to say. Let the champions of this

principle come forth with all the fury of galvanic agency, and still, hydrogen, as this principle, will do little more than float aloft in the atmosphere of fancy. There may it stay, till driven by some more adverse wind, it shall be dashed at last on the rocks of perdition.

Where was philosophy and reason, when inflammability, or the power of burning was consigned to one solitary agent? I challenge the whole host of opponents to the antiphlogistic system to adduce one single instance in all nature, in which any body separately possesses an absolute principle or quality. When we speak of the properties of bodies, as taste, smell, &c. we do not mean that any of them possesses a positive quality. They are merely sensations or effects resulting from the actions of those bodies on our organs of taste, smell &c. Inflammation, like odors, is the result of relative circumstances and not the product of a single agent.

The Professor says, "when we find a peculiar principle uniformly enter into the composition of one certain class of bodies, it can scarcely be deemed visionary to attribute to its presence, some common property of that class. This common property of that class, then, is combustibility; that common principle is hydrogen." This however, is by no means such reasoning as can be admitted, because the premises of the author are not granted. He has not proved, the existence of hydrogen as a common principle in combustibles, and there is good ground for believing that its existence is often

merely adventitious. Therefore the Professor should recollect, to take nothing as granted, which has not been proved.

The Professor is of opinion that the reduction of metals from their oxydes by hydrogen, is effected by double affinity, and in a note he remarks, that such conjecture is in conformity to one of the laws of affinity, laid down by Mr. Berthollet.

Now, I would beg leave to differ from the Professor on this point. In the first place, I deny that it is a case of double affinity and in the second place, whether simple or compond, Mr. Berthollet's laws have nothing to do with it. The particular law alluded to, does not operate in every case of chemical action, but is restricted in agency. By it we learn, that chemical action may be and often is influenced by the quantities of matter brought within the sphere of action. But when we reduce metals by hydrogen, we do not force the action by increase of mass, by no means. The action is plain and simple and no man has ever attempted to make any demonstrations capable of disproving that simplicity of action.

"It must be remembered," says the Professor, "that all the bodies capable of reducing metallic oxydes possess this principle, hydrogen." Now I deny this with equal force; all such bodies do not possess hydrogen; nay further it is conjecture and delusive experiment that has found it in many bodies. "The reduction of metals by charcoal," says our author, "is supposed to be effected by the charcoal uniting to the oxygen of the oxyd, and

flying off as carbonic acid gas, leaving the metal pure; but "says he" what becomes of the oxygen of the charcoal? I answer; prove first, that the hydrogen which is adventitious, is essential to metallic reduction. I say, it is not. The Professor supposes "that the hydrogen goes to revive the metal, part of it at least, while the remainder adheres to the charcoal to render the base capable of acidification." Why one would suppose that the charcoal in these cases was nothing but a mass of condensed hydrogen.

I would beg leave here to state, that in my opinion, the existence of hydrogen in charcoal has been completely disproved. In an experiment made by *Dr. Woodhouse with oxyde of iron and charcoal in which these substances were exposed to a red heat, the metal was not revived, nor did hydrogen appear in any shape; as was proved by Mr. Cruikshank. Now had the charcoal have contained hydrogen, this would have been evolved in form of gas. But no product of this kind was obtained. The Phlogistians, a priori, would tell us that the hydrogen would combine with the calx in order to reduce it, but no reduction in this case was effected. Then I ask where is the hydrogen of charcoal?

The Professor says "analogy supports us in the belief of an inflammable principle. There is

* The Doctor once thought this experiment an objection to the Lavoiserian system. But as soon as Mr Cruikshank investigated the subject and published the results of his inquiries, the Doctor recanted, and admitted the justness of subsequent discoveries.

"says he" a common principle of repulsion in caloric, of vision in light and of sound in air." This is truly strange philosophy; I aver there is no principle of repulsion in caloric, of vision in light, or of sound in air. To grant the existence of a repelling power in particles of caloric, is to grant spontaneous motion to matter, is to say that matter moves itself. To admit a principle of vision in light, is to depreciate the organs of seeing, as useless. Vision means the power or act of seeing, and does light possess this property? certainly not. Again of sound; how can a principle of sound be ascribed to air, since like light and caloric, it is only capable of being acted on? So with the principle of acidity; there is in fact no such thing, in any agent whatever, for acidity always results from relative causes.

"The products of combustion will not burn," says our author, "and why? They have either lost this inflammable principle, or it is so modified by the process of combustion as to be no longer governed by the same laws of affinity that previously controlled it." Now water is a product of combustion; such products according to the professor, will not burn, because they have lost their hydrogen; therefore in conformity to the strictest rules of logic, we say, water does not contain hydrogen !!! and why? because in combustion, the inflammable hydrogen is lost ! Into what errors would such conclusions leads us? they are dangerous and must be avoided.

The Professor tells us "that although oxygen increases the absolute weight of metals in a state

of oxidation, yet it lessens their specific gravity;" he therefore infers it be a principle of levity. Why not, I ask; call it a principle of weight, if the fondness of a principle in every thing must be gratified. If a principle at all, it is equally so with regard to weight as levity.

The professor attempts to reconcile the contending theories by introducing the comparative weight of hydrogen and oxygen, and endeavoring to make it probable that a small portion of hydrogen actually escapes during the combination of oxygen with the metal. Hence he infers that as hydrogen escapes, it does so as a principle of inflammability. But no man will ever accede to such conjectures, when supported neither by fact nor probability.

The difficulty started by the professor relative to the production of hydrogen gas from sulphuric acid, iron and water, is merely imaginary. No demonstrative evidence can be adduced against the simple mode of explaining this process, none has been offered. Then why should we reject simplicity, merely to gratify vain speculation? There is as little reason why the action should be compound, in this case, as in the production of hydrogen gas from the decomposition of water in a gun barrel.

"The most powerfully concentrated solar rays," says our author "produce no flame in incombustibles. They fuse and volatilize, the hardest incombustible, 'tis true, but no flame follows; yet caloric and light are both present in the concen-

trated rays, as well as probably in the body exposed to them; but the defect of this inflammable principle (meaning hydrogen) as a constituent of of the body, precludes effectually the possibility of flame." Here let us pause a moment and inquire what agents are necessary to effect a combustible process. Oxygen gas and a combustible at least, are indispensable, yet the author in the above lines has lost sight of oxygen gas altogether. This agent is the efficient cause of the incapacity of incombustible to burn. What is combustion, but the union of oxygen with a base; and if this be true, what have we to do with hydrogen to solve the difficulty, when we know that the base has combined with its *maximum* of *oxygen*, and *therefore* cannot burn.

The professor has attempted to prove the importance of hydrogen in the formation of acids, and has called it a *sine qua non* of acidity. The supposition that hydrogen is an agent in acidification is not new; and I believe it should be ascribed to some one who wrote previous to Dr. Priestley. This illustrious character in some of his writings, has hinted at something of the kind. In the 4th volume of the American Philosophical Transactions, Dr. Priestly says "Both iron and zinc, especially the latter, give out much inflammable air in pure water, and yet that water acquires no acidity." From this it would appear that the question had been argued long ago, but that Dr. Priestley did not believe hydrogen so indispensable to acidity, even when evolved in large quantities.

I agree with the profesor, that oxygen is not the sole cause of acidity; nay I go further and have already endeavoured to prove such a thing impossible.

" Hydrogen" says the Professor " is one of the simple combustibles; indeed the only one." But I am induced to believe that there is as good reason for calling carbon a simple substance. I would ask, what progress is made in science, by labouring to prove hydrogen a *sine qua non* of acidity; since in whatever compound it necessarily exists, it must be a *sine qua non* of that compound. But hydrogen does not exist in all acids, therefore the term of *sine qua non* as applied to this cannot extend to all acids. In oxalic, malic acids, &c. we find oxygen; carbon and hydrogen, each of which is absolutely necessary to form these acids. Now I might say with propriety, if the professor's argument be solid, that carbon is a *sine qua non* of acidity, because forsooth, I find it in a few acids; yet carbon does not exist in all acids, neither does hydrogen.

The professor says " the reason why oxygen and hydrogen when combined, do not form an acid, is because a base is wanting." But this I conceive, is an unfair mode of reasoning, since all circumstances should be taken into consideration. Mr. Davy has supposed that nitrogen is an oxide of hydrogen, and I believe the professor embraces this sentiment.

Dr. Priestley, long ago, was of the same opinion, and supposed that nitrous acid was formed by

oxygen combining with the hydrogen existing in the compound nitrogen. So that if the professor would only adopt this sentiment, he would then have one objection to his theory, obviated: that of nitric acid being supposed to contain no hydrogen.

"The frequent suppositions" says our author, of hydrogen being the base of muriatic acid, at least render it probable that it enters its composition." Why to be sure, if supposition is to be the means of proving a point so difficult of solution, it would be no difficult matter to prove any thing however absurd.

Supposition was the reigning epidemic in the days of alchemy, but how little did it effect in the desirable object of converting metals into gold! with respect to the simple nature of oxymuriatic acid, I need only say that Mr. Murray has satisfactorily confuted this opinion. In fact the very formation of muriates by combustion of metals in oxymuriatic acid gas is sufficient to overturn all that has been said in favour of its simple nature. True, the professor would say, the oxymuriatic acid gas contains water and it is the oxygen of this and not of the gaseous acid that oxidates a metal during its combustion. But then let me ask, pray why is not the product an oxymuriate, and not simply a muriate? this is but one of a thousand arguments that might be adduced to disprove the modern, yet most ancient theory of oxymuriatic acid.

Potash is said to be an oxide of potassium, and I will for a moment admit it. This potassium as

a metal, must according to the professor contain hydrogen; yet when exposed to oxygen, an acid is not formed, but an alkali, although oxygen, hydrogen and a base be present, as in other cases. But how does the professor evade this difficulty? he says, "if either hydrogen or oxygen be defective, acidity does not result; if by any means, the hydrogen be displaced by oxygen, the property of alkalescence becomes apparent." This is begging the question indeed. A favorite position is first laid down, and then arguments are suited to the particular circumstances of the case. For the professor does not attempt to prove by experiment, that hydrogen has been displaced by the oxygen; but the formation of an alkali and not an acid from potassium, would better favor this conclusion, and hence the inference is made.

The professor acknowledges ammonia an objection to his theory, because in it are found oxygen, hydrogen and nitrogen: yet no acid is formed. But I would beg leave to remark that this is not the onlyinstance in which these three substances combine without producing acidity. For example; in vegetable oxids, as sugar, we find oxygen, carbon and hydrogen. These cases, however, are easily explained by the antiphlogistic system, which regards the formation of water, oxides, and acids, as the mere combination of oxygen with different bases, invariable proportions.

The professed object of the essay under consideration is to reconcile the contending theories, and thus to make a kind of *tertium quid* of the

Phlogistic and Antiphlogistic systems. But I must stand opposed to such an union, conceiving it to be by no means justifiable or useful. There is nothing in my opinion radically wrong in the whole antiphlogistic system, except it be, the admission of a principle of acidity. Its principles embrace every thing relative to metals and their oxides, which Mr. Davy has investigated. Why should we admit the phlogiston of former days or the hydrogen of modern times, which are the same, as necessary in all cases, either to acidity or combustion? or rather, why should we yield opinions conformable to correct philosophy, and take in their place, a mongrel sort of theory, which has neither reason nor truth to support it? I believe acidification to be one of the most intelligible processes in nature or art. What is an acid, but an assemblage of several kinds of matter, so arranged as to constitute an homogeneous compound. And is it asked, to what I attribute the difference in acids? I answer, it is dependant on the variation in quantity of the different matters entering their composition. It has been conjectured and that with some probability, that all matter was originally capable of being resolved into three great constituents, oxygen, metallic base and electricity. Hence, if this be true, we may attribute the immense variety in acids to the infinite modifications of these original substances. We see something like this on a smaller scale, in the different compounds of nitrogen and oxygen, and in many other cases.

The description I have given of an acid, besides being simple, is adequate to explain all the phenomena of acidity in different acids. Oxygen has been deemed so indispensably necessary to the formation of an acid, that some acid bodies have been considered as anomalies because oxygen could not be detected in them. Now the view I have taken of the subject will obviate this difficulty entirely. For although I believe that oxygen does probably exist in every body possessing acid properties, yet I do not consider that opinion impaired by one or two apparent exceptions, in which oxygen has not been found. Prussic acid is said to be a compound of carbon, hydrogen and nitrogen, each of which is equally indispensable to constitute it an acid. Now one of these is as much entitled to the appellation of acidifying principle, as the other, if such a term were at all philosophical. But it is sufficient to know that these articles under certain circumstances of quantity and condition are capable of producing prussic acid.

When men pretend to reason in science, they should reason as philosophers. There are a thousand circumstances within the sphere of chemistry, which would appear absurd, were I to reason without some little regard to the philosophy of the science. Thus I would suppose it impossible that two gaseous bodies could by union, produce a solid, or that two solids by simple trituration could be converted into a fluid. But reasoning as I ought, these otherwise apparent impossibilities, are no more than mere common place occurrences.

With regard to an inflammable principle, I think proper to say that there is as little reason for retaining it as the principle of acidity. Hydrogen as well as oxygen would seem according to the professor, to be a *sine qua non* of combustion, as well as of acidity. But pray, why not call the combustible itself a *sine qua non* of combustion? for I believe no one supposes the process can go on without it. Matter, we know has a capacity to be acted upon, but not a principle of action. An alkali has a capacity of being converted into a neutral salt, by union with an acid, but it contains no principle of a neutral salt; and with as much logic may it be said, that a combustible contains no principle of combustion, or inflammability in itself. What is a neutral salt, but the result of the mutual action of an acid and an alkali? and what is combustion, but the effect of the mutual operation of oxygen gas, in some shape or other, and a combustible? The reasoning in both cases is conformable to the opinion of philosophers, that matter has a capacity of being acted upon, but that it cannot be its own mover. If oxygen gas and a base produce acidity, why cannot oxygen gas and a base produce also the phenomena of combustion? why in the one case should we call to our aid an agent as indispensable when in the other we explain the process by more simple means? The difficulties started are ideal, and like Phlogiston itself cannot be demonstrated or even rendered probable.

With these remarks I leave the essay for the present. Let the curious examine and judge accordingly. "Knowledge" says a great man, "is power;" and error in science is its greatest curse. The road to truth may be long and tortuous, but he that gains admission into the noble edifice, will never look back with regret, on the difficulties he has surmounted; on the contrary they will serve as a lasting source of gratification, and add new lustre to his triumphs.

EXPERIMENTS AND OBSERVATIONS

ON PUTREFACTION.

By John Manners, M. D.

FROM reading a paper upon the vinous and putrefactive fermentation by Gay Lussac in a late number of Nicholson's Journal, in which the author according to the general opinion of chemical philosophers, contended that the access of atmospheric air or oxygen gas, was a *sine qua non* of the process, I was induced to institute the following experiments on *putrefaction;* by which I have proved (as I conceive) beyond the possibility of exception, that oxygen is not only unessential to the putrefactive fermentation, but has, when in actual

contact with the putrefying substance no influence on that process.

August 10th 1812 I secured some fresh muscular flesh (a portion of lamb) in the bottom of a glass jar and inverted it over distilled water, observing that the water within the jar, was precisely on a level with that which was external; that any absorption of either of the components of the intercluded atmospheric air, might be noted by a corresponding absorption of water within the jar.

Fahrenheit's thermometer stood at 70, at which temperature it was kept during the experiment.

That the distilled water was perfectly free from any oxygen gas, I proved by Mr. Dreissen's method viz. I tinged a portion of the same water with *litmus* and passed nitrous gas through it; which Dr. Priestley proved would combine with the oxygen and be coverted into nitric acid, which would change the litmus red.

Dr. Thomson says, however, that this is not a critical test, and that the litmus will not be changed unless there be an unusual quantity of oxygen gas present.

Upon the discovery of this property of nitrous gas by Dr Priestley he founded the first eudiometer, which has been since improved by Falconer, Fontana, Cavendish, Ladriana, Magalan, Van Humboldt, Ingenhausz, Dalton and Gay Lussac, and contributed so much to extend the bounds of phil sophical knowledge. Before this important *era* the only eudiometer in the hands of the philosopher was a *sparrow*, a *mouse*, or a *taper*. Since, how-

ever, others have been devised, as the sulphuret of iron by Scheele the hydro-sulphuret of potash by De Marti, the rapid combustion of phosphorus by Humboldt and Seguin, the slow combustion of phosphorus by Berthollet, the green sulphate of iron impregnated with nitrous gas by Mr. Davy, and the detonation of hydrogen gas by Valta.

The jar remained three days, during which time the flesh had undergone the putrefactive process as was evinced by the offensive odour emitted. But at no time could I observe any absorption of water within the jar: except there was a corresponding reduction of atmospheric temperature, and in consequence a condensation of the intercluded air. But Dr. Priesttley in a similar experiment found a small augmentation of air within the jar: as I have in subsequent experiments.

The confined air was analysed with the eudiometer of Humboldt, but not found to differ from atmospheric air, in the proportion of it oxygen, and nitrogen.

The state of the barometer, however, was not attended to in this experiment, which renders it liable to exception. Neither did Dr. Priestley attend to the state of the barometer in his experiments; or if he did, he omitted to mention it.

I repeated this experiment over the mercurial bath.

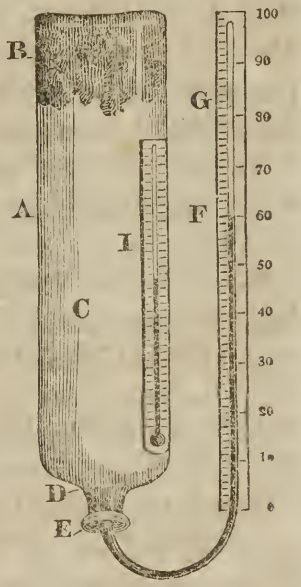
The thermometer as before stood at 70, and the barometer at 29,1 inches.

The experiment was continued three days, when the putrefactive fermentation had taken place, as

was evinced by the odour emitted: But there was at no time any absorption of mercury within the jar.

Upon examining the included air after the experiment with the eudiometer, it was not found to differ from atmospheric air.

But to magnify and render more conspicuous, any absorption in consequence of a diminution of the included atmospheric air, by the combination of its oxygen with the animal flesh, I invented an instrument which I shall now describe.



I took a cylindrical bottle (A) perfectly transparent, and put half a pound of muscular flesh (a portion of the diaphram of a bullock) in the bottom of it, and secured it there. (B) The flesh was taken while warm and cooled under mercury to prevent the access of air. To the bottle was adapted a cork which was perforated, and a bent tube passed thro' the perforation, the other end of which was hermetically sealed. (E) Some mercury was then put into the bottle; the bottle corked, and made perfectly air tight by lu-

ting and sealing. The bottle was now inverted. The mercury filled about two inches of the neck of the bottle, (D) and was made to pass up the glass tube by heating it, and expanding the air and thus expelling a portion of it, to a proper distance. (F) In this situation the bottle was put to rest in a fixed position.

A thermometer (I) was included within the bottle in order to note its temperature.

The bottle and curved tube in some measure represented Mr. Leslie's differential thermometer. Here barometrical influence was perfectly excluded. And as the variations in the temperature equally affected both the air included in the tube, (G) and that in the bottle, (C) it is evident that thermometrical influence could not effect the experiment.

To the tube was adapted a graduated scale which would mark any rise or fall of mercury in the tube.

Now it is clear that the smallest diminution of air in the bottle would be marked by a corresponding fall of the mercury in the tube, the calibre of which was not more than one line. Or on the contrary, any evolution of gas would raise the mercury in the tube.

The apparatus remained three days without any change of mercury in the tube. On the fourth day the mercury began to rise and continued to rise untill the experiment was suspended; which proves that there was no absorption of oxygen gas by the putrefying substance.

The thermometer included in the bottle stood at 60 during the experiment.

This apparatus is easily constructed and may be used for many similar purposes, as a gasometer.

As from all these experiments it appeared that no oxygen gas was absorbed by the putrefying substance, I determined to exclude the atmospheric air altogether. This I attempted *first* by the ollowing experiment.

I put some fresh meat into an eight ounce phial; filled it with mercury; placed it in the pneumatic cistern; it was then filled with carbonic acid.

In this situation it was kept three days, at the end of which period the flesh was found to have undergone the putrefactive fermentation: though all air except carbonic acid was excluded. Yet Sir John Pringle and Dr. Mc Bride (and the latter from actual experiments) contended for the antiseptic powers of fixed air. The thermometer during this experiment stood at 70.

But as my object was to exclude oxygen gas, and as carbonic acid contains that as one of its components, I thought it not impossible but that the animal matter might abstract a portion of the oxygen from the carbonic acid, and convert it into carbonic oxyde: as is the case with *iron*, *zinc*, *tin*, and certain other metals.

I therefore repeated the experiment in every circumstance, as before, except that I substituted *hydrogen* for *carbonic acid* gas, but with precisely the same result.

Putrefaction went on as well as in any of my former experiments.

I tried sulphuretted hydrogen and nitrogen gases in the same manner with the same result.

I then fell upon a second method of excluding atmospheric air and oxygen gas.

I took an eight ounce phial and put six ounces of fresh beef (a portion of the diaphram) in the bottom of it, and secured it there. In procuring this beef I was so careful as to go to the butchers myself and have it cut off the moment the animal was dead. Upon this meat while thus warm, and not affected by the external air, I placed a column of mercury by filling the phial with that fluid.

The phial was corked and to the cork was adapted one leg of a syphon which perforated the cork; all was made perfectly air tight by luting and sealing wax. The syphon was filled with mercury completely, and passed into the mercurial cistern. Over this was placed a glass vessel filled with mercury and inverted; in order to collect any gas that might come over. There was now a complete column of mercury from the meat to the top of the vessel inverted in the cistern. My object in the *first* place was to prove by the first phial containing the meat covered with a column of mercury, whether putrefaction could take place in that situation where the possible access of air was cut off by the mercury. My object with the syphon and other apparatus was to collect and examine the product, if putrefaction proceeded. The thermometer stood during this ex-

periment at 70. In about three days the putrefactive process was evidently going on.

These experiments were sufficient to satisfy me that atmospheric air or oxygen gas, is so far from being essential to putrefaction, that it has no influence on that process when it has free access to the putrefying substance.

I mentioned to my friend Dr. Thomas D. Mitchell my experiments and their result, who has since repeated and confirmed them. He has also extended those experiments to the *vinous* fermentation, but the result of his experiment on this subject, he is not yet able to determine.

I therefore agree with Dr. Mitchell that putrefaction depends on a distruction of the equilibrium of attractions which exists in the elementary principles of which the animal substance is composed in a healthy state, occasioned by the loss of vitality in consequence of which new compositions and decompositions ensue.

My next object was to examine the products of putrefaction which had taken place without *extrinsic* oxygen.

The *first* product was a *bloody scum.* The *second* was a *transparent gas*, possessing the *transparency*, *elasticity*, *dilatability*, *compresibility*, and other mechanical properties of atmospheric air.

As the gaseous products of putrefaction had never been collected and chemically examined I thought it an object of importance to give it a *careful* and *critical* analysis.

I therefore proceeded to examine it in the following manner.

As it has been the unanimous opinion of chemical philosophers who have written on the subject, that ammonia is generated and is the principle product of the putrefactive process, I first tested it for that substance.

1st. By passing a peice of litmus paper reddened by vinegar into a vessel about half filled with this gas over mercury ; no change ensued.

2d. Some of the gas was passed through an infusion of litmus reddened by vinegar;—no change.

3d. I filled a vessel with mercury over the mercurial bath and displaced about half of it by passing up an infusion of litmus reddened by vinegar. After which I passed up the gas, which was readily absorbed untill it was strongly impregnated and the gas had accumulated in the top of the vessel ; —no change.

4th. 5th. 6th. I tested it with turmeric in all the three ways in which litmus reddened by vinegar was used ;—no change.

7th. I passed up a piece of wet sponge by means of a wire but there was no perceptable absorption of the gas by the water contained in the sponge.

8th. The sponge was withdrawn and washed in a solution or *sulphate of copper*;—no change.

9th. I passed a solution of *sulphate of copper* into a vessel filled with mercury over the mercurial cistern untill it was two thirds displaced by the solution of copper. I then passed up the gas

until the substance was thoroughly impregnated with it and had accumulated in the top of the vessel; —no change.

10th. Carbonic acid was passed up into a vessel containing this gas; no chemical change. (except we call, with Mr. Berthollet, the admixture of gases, chemical dissolution.) The carbonic acid produced an augmentation of the bulk of the gases proportional to its quantity.

11th. Muriatic acid was passed up into a vessel filled with this gas over mercury;—no change.

These experiments were abundantly sufficient to prove that there was no ammonia in the product of putrefaction at least when it takes place without the influence of external causes.

Secondly, I tested it for oxygen in the following manner.

1st. A piece of phosphorus was passed up into a vessel filled with this gas, and standing over mercury. The phosphorus was fused, and became perfectly fluid, (floating upon the surface of the mercury,) by pouring boiling water upon the vessel. But there was not the slightest appearance of combustion.

2d. Water was now passed into the same vessel which was tested for phosphoric acid by litmus; no change.

3d. Nitrous gas was passed into a vessel filled with this gas over mercury; no change, except in the bulk, proportional to the gas added.

4th. Water was now passed up into the same vessel and tested for nitric acid by litmus; no change.

5th. A mouse was passed into a vessel of this gas, which instantly died.

These experiments were deemed sufficient to prove the non existence of oxygen.

Thirdly, It was now tested for sulphuretted hydrogen.

1st. A piece of silver was placed in a vessel of this gas which was not blackened or converted into a sulphuret, when withdrawn. A piece of silver was also kept in water highly impregnated with this gas, and another piece was placed at the end of the tube from which the gas was disengaged and with the same result.

2d. The gas was passed through a solution of nitrate of silver ;—no change.

3d. A vessel was filled with mercury over the mercurial trough, and displaced by passing up a solution of nitrate of silver untill the vessel was half filled with it. After which the gas was permitted to pass up, as it was disengaged from the putrefying substance through the mercury and through the solution of silver until it was strongly impregnated with it and had accumulated in the upper part of the vessel ;—no change.

4th. This gas was passed through a solution of sugar of lead (acetas plumbi) in the same manner as experiment 2d. no change.

5th. A solution of acetate of lead was impregnated with the gas in the same manner that nitrate of silver was in experiment 3d. ;—no change.

These tests were sufficient to prove that no sulphuretted hydrogen was formed.

Fourthly, I tested it for carbonic acid.

1. By litimus paper; reddened.

2. By passing the gas through a solution of litimus; reddened.

3. I passed up a portion of an infusion of litmus into a vessel filled with mercury until the mercury was three fourths displaced and then passed up the gas from the putrefying substance;—the litmus was reddened.

4th. I filled a vessel with mercury over the mercurial bath and passed up lime water. After which I passed up the putrescent gas. The lime was precipitated.

5th. The precipitate obtained in a number of experiments, effervesced with acids as the muriatic, sulphuric, nitric, oxalic acids, &c.

Consequently the gas was carbonic acid. Probably holding a fœted oil (or some of the animal matter) in solution, to which it owes its offensive odour.

From the six ounces of animal matter I have already collected more than 100 cubic inches of this gas.

Other professional engagements and the approach of cold weather prevented my making these experiments more complete. But the subject will again be resumed when favourable weather returns. I therefore submit them to the society, imperfect as they are.

OBSERVATIONS

On the formation of muriate of potash in the process of preparing the hyperoxymuriate of potash by WILLIAM HEMBEL JUN. ESQ. *of Philadelphia.*

THE formation of muriate of Potash, in the process of preparing the hyperoxymuriates of potash does not appear to me satisfactorily explained. Berthollet supposes, that the oxymuriatic gas is decomposed during the process, one portion of the gas losing the whole of its oxygen, is reduced to simple muriatic gas, whilst the other portion combines with the separated oxygene and is converted into hyperoxymuriatic acid gas. To me it appears more probable, that muriatic, as well as hyperoxymuriatic gas are evolved at the same time, during the effervescence of the muriatic acid and that each in their disengaged state combines with the dissolved salt, according to laws, which govern their respective affinities.

When muriatic acid is added to manganese, an action takes place. The temperature of the mixture is raised, and oxymuriatic gas is evolved in proportion to the strength of the acid employed. But the action, is not with the whole of the muriatic acid employed. It can only be with the lower stratum, or as much of the acid as is in contact with the manganese. The supernant acid remains inert until the whole, or part of the acid of the substratum is volatalized. In fact it appears probable, that the parts of the supernatant acid are brought into action, only in proportion as the substratum is driven off by the heat of the mixture, or of the fire employed: from whence it results that part of the muriatic acid of the supernatant acid, must rise along with the oxymuriatic acid gas, and both passing into the saturating vessel, each must combine with the potash, in proportion to their respective affinity. The muriate of potash remaining in solution, whilst the oxymuriate crystalizing falls to the bottom of the vessel.

Further, it appears highly propable, that the proportion of muriatic gas evolved during the process considerably exceeds the oxymuriatic for the following reasons.

The strongest muriatic acid which can be obtained the specific gravity of which is 1.196 consists, according to Kirwan of 25.28 real acid and 74.72 water.

Now, as the whole of the acid affused over the manganese, cannot come in contact with that substance at the same time, and as the water with

which the acid is combined, remains after the acid is driven off. I think it highly probable, that before the process is nearly finished the acid is too much reduced in strength, to be capable of disengaging any oxygen from the manganese.

To illustrate the above opinion, let us suppose, 500 parts of muriatic acid, are affused over any quantity of manganese; and that the 500 parts of acid are divided into 5 strata, distinguished by the letters ABC, DEF, and that the strata *acts only* in succession on the manganese.

Now 500 parts of the strongest muriatic acid, are composed of 373.60 water and 126.40 acid. Consequently each stratum would be composed of 25.28 acid and 74.72 water. Hence, the decriment of the strength of the acid during the process would be as follows.

	Acid.	Water.
At the commencement of the process there would be	126.40	373.60
After the stratum A was deprived of its acid, there would remain	101.12	373.60
After the stratum B	75.84	373.60
C	50.56	373.60
D	25.28	273.60

E and the remainder, would be the water with which the whole acid was combined.

ANALYSIS

OF THE

BORDENTOWN (N. J.) SPRING.

BY SAMUEL F. EARL.

A small quantity of the water from this spring was sent to me (some time since) for examination, when the following experiments were made; but business of more importance at that time, before the Society, restrained me from presenting the analysis.

The 1st. experiment I made, was with a view to ascertain whether the water contained any earthy or metallic matters; a solution of *potash* was therefore added, which gave a brown tinge and precipitate.

Experiment 2d. *Tinct. of galls* changed it to a purple and black precipitate formed by standing.

Experiment 3d. *Prussiate of potash* made but little change until one drop of sulphuric acid was added, when a deep *blue* color was immediately produced.

Experiment 4th. A portion of the water was boiled a few minutes: tinct of galls now added, produced no change of color; during the boiling a *yellowish brown* precipitate was deposited on the sides and bottom of the vessel.

Experiment 5th. *Sulphuric acid* was added, which caused an effervescence, or extrication of gas.

Experiment 6th. A solution of *acetate of lead* produced a *white* precipitate by standing.

Experiment 7th. A solution of the *muriate of barytes*:

Experiment 8th. A solution of the *nitrate* of *silver*:

Experiment 9th. A solution of the *oxalic acid*, were all separately added, but made no perceptable change.

Experiment 10th. I evaporated to dryness, 8 ounces of the water; the residuum weighed 2.75 of this 1.00 was an oxyd of iron; the remaining 1.75 I infered from the preceding experiments was earthy matter.

RECAPITULATION.

The first experiment proved the water contained *earthy* or *metallic* bodies in solution.

Experiments 2d. 3d. fully proved this metallic body to be *iron* (the sulphuric acid was added in experiment 3d. to saturate, if *present*, any uncombined earth.)

From experiment 4th. I found the acid which held the *iron* in solution, to be *volatile.*

Experiments 5th. and 6th. proved beyond a doubt, this *volatile acid* was the *carbonic acid gas.*

Experiment 7th. 8th. and 9th, the first, a test for *sulphuric acid*, the second for *muriatic* acid, and the last for *lime*, proved their nonexistence.

By experiment 10th. I found 1 pint of the water contained 2 grains of an oxyde of *iron*, a far greater quantity than the Pyrmont or Cheltenham, both celebrated chlybeates. Not having any litmus, I inferred from the circumstances attending drinking the water, that it contained a large qusntity of *uncombined carbonic acid.*

From a review of this analysis, I think we may justly rank the water of the Bordentown Spring, among the highly *carbonated chalybeates* of the *United States.*

REPORT

Of the Committee to whom was refered the analysis of certain ores, presented through the medium of Thomas Brientnall Esq. to the Columbian Chemical Society.

Mr. President,

YOUR committee taking into consideration, the many troublesome and tedious processes, attending an accurate analysis; the very small quantities of the specimens, and also being told, that the wish of the gentleman who presented them, was not a knowledge of the component parts, and exact quantity of each part, but of that (if any) with its quantity, which could be worked to advantage, report as follows:

Specimen No. 1. Granular and very brittle; color brownish black, interspersed with small specks resembling the Hematites. When pow-

dered, attracted by the magnet specific gravity 3.817. About two ounces of the *acid muriatic* were poured on fifty grains *finely powdered*, and after digesting a sufficient length of time, decanted; fresh portions of the acid were repeatedly added, until by testing it with the *prussiate of potash* and the tinct. of galls, it was found no more of the ore could be taken up. The several decantations and washings, were then mixed, and saturated with a concentrated solution of the *sub. carb. potass.* by which process the oxyd of iron was precipitated, together with a small quantity (hardly perceived by the addition of the aq. ammon. or immersing polished iron) of copper. The precipitate thus obtained was dried, and subjected in a crucible (with powdered charcoal,) to a strong heat; after cooling it was carefully taken out, and now weighed 49½ grains, equal to 35 grains of *metallic iron.*

Specimen No. 2. Very compact, and sonorous, of a dark slate color; specific gravity 4.020.

Twenty five grains finely powdered, were put into one ounce of the acid muriat. then digested and decanted: as before a few drops of this solution, were mixed with a wine glass of water, and the aq. ammon. added, ammoniaret of copper was formed immediately. The sol. pruss. potass. was then added in the same manner as the aq. ammon. and produced a blue color (or the pruss. Ferri.)

The whole solution was saturated as that of No. 1 with the concentrated solution of the sub. carb. potass: the obtained precipitate, after drying, was redissolved in the acid. muriet. The cupreous portion

of the solution amounting to $4\frac{1}{2}$ grains was taken up by polished iron, 22 grains of the ferruginous oxyde were left, which gave $15\frac{3}{4}$ grains of metallic iron and $4\frac{1}{2}$ of copper.

No analysis was made of the remaining specimens; their specific gravity, being so low, (none exceeding 2.300) and to appearance, precisely the same nature as specimen No. 1.

SAMUEL EARL.

Aug. 19th. 1812.

REMARKS

ON THE ATMOSPHERE.

Read before the Columbian Chemical Society, *by* THOMAS D. MITCHELL M. D. *Fellow of the Academy of Natural Sciences of Philadelphia. Honorary member of the Columbian Chemical Society of Philadelphia, &c.*

THE discussion of subjects, respecting which the opinions have been various, is certainly of as much importance, as speculation on a new topic. In the one case, we have presented to us the ingenuity and industry of the many who have preceded us, while in the other we have no better inducement to an exertion of talents, than the fanciful hypothesis of some daring spirit of research.

What then is our prospect in either case? If unabating zeal for the truth profit us not on the one hand, we are as apt to be perplexed and disappointed on the other. But while I argue for the importance of repeated investigations of antiquated subjects, I am not the less sensible that as truth possesses but one front, we should be indifferent in what path we direct our steps to obtain it.

These remarks having been premised, I beg leave to call the attention of the society a few moments to the subject of the atmosphere.

It may not be amiss to inquire in this place what is meant by the atmosphere in contradistinction to atmospheric air. For however strange it may appear, there are those who consider the one as distinct from the other. By the atmosphere, I mean that fluid mass apparently homogeneous in its nature which is found on every part of the earth we inhabit; the properties of which are levity, elasticity, invisibility, &c. &c. This acceptation of the term is perhaps universal, yet some define atmospheric air differently. It is said to consist of nitrogen and oxygen only and that carbonic acid gas &c. are merely adventitious. But as the term gas has been substituted for that of air, it seems most correct to give to each of the gases in the atmosphere, the name of atmospheric air. Nor is there any impropriety according to this arrangement in calling the whole mass, atmospheric air. For as each different gas in the atmosphere is necessary to constitute the mass itself, so each must be an atmospheric air.

The point more particularly to be investigated is, whether the atmosphere is a simple mixture, or a chemical compound. This though apparently a subject not difficult of explanation, has been much disputed. What are the grounds for supposing it to be a simple mixture? It has been said that a separation of the gasses oxygen and nitrogen ensues after a considerable lapse of time, if rest be allowed.

Again it has been asserted that the same given quantities of oxygen and nitrogen would, under different circumstances produce the very opposite compounds of nitric acid and atmospheric air. The two compounds being so widely different and no one having denied the compound nature of nitric acid, the atmosphere has been deemed a simple mixture. Let us for a moment test these by hypotheses.

In the first place then I ask, is the supposed separation of the component parts of the atmosphere a case of simple disunion, or chemical decomposition? I am of the latter opinion. We are told that a quantity of atmospheric air by long standing in a confined place, evinced a separation of its constituent parts, and that nitrogen and oxygen were found in the vessel in a separate state. But what does this prove? It is in my opinion an argument in favor of the compound nature of the atmosphere, that is of its chemical composition. True, no evident means, no manual efforts were made use of to effect a separation. But why are the repeated changes of temperature disregarded, why the den-

sity of the air unnoticed, and why the never ceasing agency of moisture and dryness neglected to be mentioned? Are these of no consequence in chemical research or is their importance so trifling as not to merit the least attention? By no means; every circumstance however trivial it may appear, is ultimately of the greatest magnitude, and should never be forgotten. What shall we say of the deoxidation of metallic salts when kept in a situation exposed to the light? Are these not chemical compounds? No one pretends to call them simple mixtures, and yet the deoxidation is not the effect of mechanical operations, the powers of increased heat or of double decomposition from mixture with other bodies. In such cases there is often an actual separation of the oxide from the acid with which it was combined.

Again; the opposite compounds said to be produced under different circumstances from the same quantities of nitrogen and oxygen; have been adduced as an argument in favor of the opinion that the atmosphere is a simple mixture. But what support can this give? Surely none. In the first place I deny the position, since it has not been demonstrated that the same quantities of nitrogen and oxygen do under different circumstances ever produce the compounds atmospheric air and nitric acid. But even admitting the truth of the position. it argues nothing in favor of the opinion it is designed to support.

The gasseous oxide of nitrogen is, as all know, a compound of the same gasses of which nitric

acid is formed. Yet no one has absurdity enough to imagine that nitrous oxide gas is less a chemical compound than nitric acid. And are their properties the same? By no means. This ought to be the case howvever, if the reasoning formerly alluded to were correct, viz. that when two substances diametrically opposite to each other are formed from the same ingredients, one of them must be a simple mixture. The fallacy of this reasoning however is obvious, as the premises, themselves are false.

Having briefly considered the arguments, in favor of the simple nature of the atmosphere, I shall now offer some remarks calculated to prove that it is in fact a chemical compound.

The laws of attraction have been divided by chemists into several orders.

It is my intention here however to premise that there are properly but two kinds of attraction, viz. That by which particles of matter whether similar or dissimilar are united independant of a chemical change in either body. It may be called adhesive or cohesive attraction, the former of which terms, I prefer. The other species is that by which particles of matter are united and a total change of properties affected. It is called chemical attraction. Now under one or other of these kinds of attraction, all the operations of nature must be included Let us for a moment apply these remarks to the subject under consideration.

Is the atmosphere a simple mixture of particles dissimilar in their nature? What says the law of che-

mical action? It tells us that the bodies submitted to its operation have their properties changed. What then are the properties of nitrogen and oxygen in a separate state and what are the peculiarities of atmospheric air? If none of the peculiar properties of nitrogen exist in the compound atmosphere of which it is a part, then it follows that a chemical action has ensued by its combination with oxygen, otherwise the law of chemical action is made void.

Nitrogen gas is fatal to life, and will not support combustion. Atmospheric air is the grand vehicle of existence and an indispensable agent in the production of flame.

I doubt whether there can be such a thing as simple mixture, that is a union of two bodies, independent of all the collateral circumstances which influence chemical action; If there be indeed such a thing, I believe there is no better instance of it than the mixture of hydrogen and nitrogen gasses. By adding a portion of hydrogen gas to another portion of nitrogen, we produce no change in the properties of either unless the electric spark or some such agent be called into operation. They immediately arrange themselves according to the specific gravity of each.

But the case is very different with oxygen and nitrogen. Here a perfect union is speedily effected and the peculiarities of the components are lost in the compound.

I think from what has been said in investigating this subject, it is plain, that if chemical action be

exerted in any case, and if a change in the properties of bodies placed in contact with each other, be a proof of such action, then is the atmosphere a chemical compound and not a simple mixture.

If we should be disposed to admit the simple nature of the atmosphere, we would then be obliged to class it under the list of examples of cohesive attraction.

But how should we be able to reconcile this with the generally received definition of cohesive attraction. It is said to be that kind of affinity by which similar particles of matter are united. Now oxygen and nitrogen are dissimilar in their nature, of course, according to the above definition, the atmosphere could not be a case of cohesive attraction, and consequently not a simple mixture.

Hence it is obvious that as oxygen and nitrogen gases are different in their properties, if a union can take place between them, that union cannot be simple. How could it possibly be simple, since in all cases in which two or more bodies unite, so as to lose entirely their characteristic properties, a chemical action is necessarily implied.

I have purposely omitted the consideration of the carbonic acid gas found in the atmosphere; for if the remarks already made respecting oxygen and nitrogen be just, they are equally so when applied to carbonic acid gas. The subject is plain in itself and requires but a small share of investigation to satisfy the most sceptical in science.

A NEW METHOD OF MOUNTING

WOULF'S APPARATUS,

In which the tubes of safety are superseded, by William Hembell Jun. Esq. Communicated to the society by John Manners M. D.

Philadelphia November 20th, 1812.

Doct. Manners,

Sir,

I inclose for your inspection, a method of mounting A. Woulf's apparatus in which, tubes of safety are totally unnecessary. If it should meet your approbation, please to present it to the Columbian Chemical Society.

I remain Sir,

Your Friend,

Wm. Hembell, Jun.

THE utility of Woulf's apparatus, is considerably diminished by the necessity of a tube of safety; if streight tubes are used, the liquid is forced out of the bottles, whenever there is a considerable

accumulation of gass, whilst Welter's tubes are so costly and delicate in their structure; that they can scarcely be handled, without danger of breaking.

In the Frontispeice is exhibited a manner of connecting any number of bottles, by which the escape of gas is prevented, and the danger of contamination is obviated, without the necessity of tubes of any description.

The tube H, merely perforates the corks, without descending into the receiver A, or bottle B. The tube I, descends nearly to the bottoms of the bottles B and C. K only perforates the corks, L descends nearly to the bottoms of the bottles D, E, F & M only perforates the corks, whilst N descends nearly to the bottoms of the bottles F and G. A similar arrangement of short and long tubes are to be used, for any additional number of bottles.

To illustrate the advantage, which this manner of connecting bottles possesses over the usual method by tubes of safety, we will suppose, the operator wishes to prepare muriatic acid.

In that case, the receiver A and bottle B are left empty; a little water is put into the bottle C to condense, whatever sulphureous gas is evolved. The bottle D is left empty, and the water to be saturated with the muriatic acid gas; is to be introduced into the bottle E. F is left empty, and into G, a further quantity of water is introduced; to condense the gas, which may escape condensation in the bottle E.

On adding sulphuric acid to the muriate of soda, contained in the retort, an action will take place. The evolved gas, will pass through all the bottles as usual.

If, from engagements or neglect, the operator omits to add fresh acid when the gas ceases to be evolved, a condensation will take place in the retort. Then the liquid, in the bottles C E and G, will yield to the joint pressure of the incumbant atmosphere, (acting through the aperture of the tube O,) and the accumulated gass in the empty parts of the bottles C E and G and pass into the bottles B D and F. But as the apertures of the short tubes H K and M terminate with the lower ends of their respective corks, the liquid in the bottles does not reach those apertures; consequently, it can pass no further.

After an absorbtion has taken place, it is only necessary to excite a fresh action in the materials of the retort, the gas will then accumulate in the receiver and empty part of the bottle B. When the accumulated gas is sufficient to support a column of the liquid, equal to the highest of one leg of the tube I, the liquid will rise, and pass into the bottle C, the gas will follow and pass into the bottle D, and by pressing on the surface of the contained liquid, will force it over into E; after which, the contents of F, will be forced into G, when the process of saturation will go on as before.

It will even be advantageous to the process, that an absorbtion should take place frequently, as the

surfaces of the liquid will thereby be changed and the condensation of gas considerably accelerated.

With an apparatus arranged as the above, the writer of this, saturated thirteen pounds of water with muriatic acid gas; only that he substituted demi-johns, for the bottles D, E, F and G, his attention being frequently otherwise engaged, the apparatus, was suffered to stand for three weeks, without any injury to the product.

It can hardly be necessary to remind the manufacturer, that for nitric or muriatic acids, an iron matrass may be substituted for the glass retort. In that case, the receiver may be dispensed with, one end of the tube H, being made to enter the tubulure of the matrass, and the other end entering the turbulure of the bottle B.

END OF VOLUME I.

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